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XXI. *Experiments with a new Micro-Balance.* *By* Dr. Hans Pettersson.

Received April 12, 1920.

(Communicated by A. W. Porter, F.R.S.)

Considering the fundamental importance of accurate weighings both for physical and chemical research it is surprising how little progress had been made until about ten years ago with regard to improving the sensibility of weighing balances. A certain number of micro-balances had been constructed by Angström, Salvioni, Nernst and others; but their absolute sensibility rarely exceeded a thousandth part of a milligram. Owing to the very delicate mode of suspension chosen for these instruments they were extremely fragile, so that the maximum load which they could safely carry was comparatively small. Their not very considerable gain in *absolute* sensibility was therefore compensated by a disproportionate loss in *relative* sensibility, which contributed to make their use very limited. The most widely known of these early micro-balances, the Nernst balance, could only be charged with a few cg., which it weighed to about 0·001 mg.

Quite a new departure in the technique of micro-weighings was made in 1911, when Steele and Grant published a description of their remarkable all-quartz micro-balance.* By making the beam, as well as the weights and counterpoise, entirely from fused, non-corrodible, and practically non-hygroscopic silica, they were able to give their instrument a very high constancy of its zero. The mode of balancing it on a minute knife-edge of ground quartz made it susceptible to extremely small changes in weight, which could be counterbalanced and measured with a sufficient degree of accuracy, thanks to an ingenious method of aerostatic compensation, namely by varying the pressure of the air inside the case, so as to change the buoyancy of a small sealed silica bulb of known capacity (the "air-weight") suspended from the balance.

Given a certain dexterity on the part of the operator, these instruments could be put to excellent use, as was amply proved by the brilliant work of Sir William Ramsay and Dr. Whytlaw-Gray on the density of niton and on the atomic weight of radium.†

* Proc. Roy. Soc., A. 82, p. 580.

† Proc. Roy. Soc., A. 84, p. 536, and A. 86, p. 270.

In the course of these investigations a tiny gold capsule, after being heated red hot, was found to undergo a curious loss in weight, which it slowly regained on cooling. This effect was first supposed to be due to a liberation of air adsorbed by the metal; and when my work at University College, London, was begun in the autumn of 1911 a closer study of this phenomenon was the subject suggested to me by Sir William Ramsay. For this research the quartz micro-balance was to be used, and I was incidentally to try to improve it, so as to make it stronger and less difficult to work with, by suspending it by metal fibres instead of balancing it on a knife-edge.

The advantages of this arrangement appeared somewhat doubtful, considering the rather imperfect elastic properties of metals. Still the method had to be tried. The technical difficulties of soldering thin Wollaston wires to the platinized silica rods of the beam (an operation carried out under the microscope with the aid of a minute electrical soldering apparatus), proved to be unexpectedly great. Several

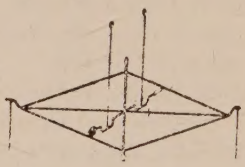


Fig. 1.

months of vain attempts finally resulted in a complete quartz balance suspended by platinum fibres about 0·01 mm. thick. The suspension, however, was entirely unsatisfactory; even at a comparatively low absolute sensibility the zero was found to undergo considerable variations.

Before falling back on the old knife-edge balance I made an attempt to suspend a beam by fine *silica* fibres drawn out from the beam itself. The first balance of this type gave very good results, and my subsequent investigations, of which a short account is to be given in the following, have been carried out with similar instruments. The properties of the fibre suspension have been studied theoretically and by experiment. The technique of the instrument itself and of the weighings has been developed. Finally, a fibre-suspended micro-balance has been used in some physical investigations.

It is not possible for me to express adequately my feelings of gratitude to my late teacher, Sir William Ramsay, under whose inspiring influence I have been allowed to work for nearly one year. Probably only those who have themselves had the privilege to work under this " prince de la science " will be able to realise it.

*The Fibre Suspended Micro-Balance.**

After about ten months spent at University College these investigations were continued in the physical laboratory of Stockholms Högskola, its director, Prof. C. Benedicks, kindly

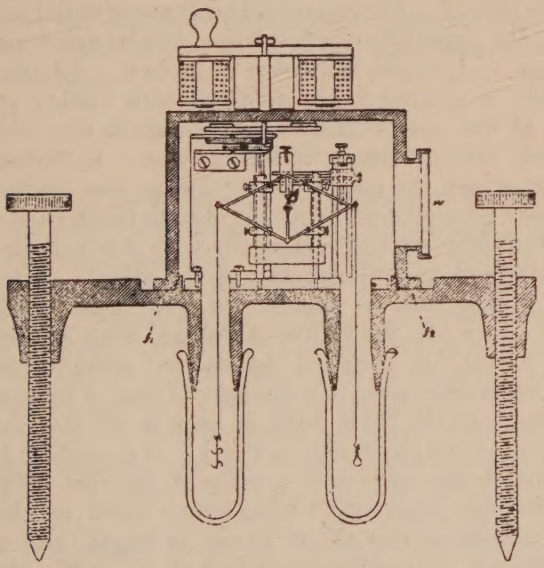


FIG. 2.—VERTICAL SECTION OF MICRO-BALANCE (TO HALF SCALE).

putting the necessary instrumental resources at my disposal. With the aid of the skilful mechanic of that institution, Mr. C. Andersson, a new type of case was constructed. Fig. 2 represents a vertical section through a similar case, made for a balance of *reduced* size by the firm Lyth of Stockholm, who are to manufacture instruments of this kind.

The balance-cases used by Ramsay & Gray had a solid brass rod, by which the arrestment was worked, ground into

* *See* my thesis :—" A New Micro-balance and its use." Göteborg's " Vet. of Vitterh. Samhalle's Handlinger," XVIth series, 1914.

a conical jacket through the floor of the case and made air tight with rubber-grease. On the new case the arrestment is worked without any material transmission, simply by rotating an " electro-magnetic key " on the top of the brass cover, so that a soft-iron anchor-piece is carried round inside by the magnetic forces. In this manner the beam can be lifted off its central fibres when not in use, or made to depend from them when a reading is to be taken.

The supports which grip the silica hooks at the ends of the central fibres can be moved by screws in order to adjust the beam to its correct position, so that the end-fibres depend straight into the centre of the detachable weight-tubes. These are ground to fit over conical brass pieces under the floor of the case, the upper side of which has a circular grove into which the flange f_1 f_2 round the cover is ground. All the ground joints are in general made air-tight with rubber grease, a leakage of less than 0·01 mm. of mercury a day being thus obtainable with a vacuum within the case. In certain investigations, where the use of rubber grease was excluded, air-tight joints were secured by means of seals made from an alloy of low fusibility, covered externally with low-volatile cement (as a safeguard against porosity of the alloy). Low-volatile cement is also used in case of the plate-glass window, W, through which the light from a Nernst lamp passes to the balance mirror, the image of the filament being focussed on a vertical millimetre scale at a distance of, say, 4 metres.

The shape of the beam itself, as used in my own investigations, is seen from the sketch in Fig. 1. It is made in the ordinary way on the surface of a piece of gas-coal. The fibres are drawn out from small silica-hooks fused to the beam. With a balance of this shape, 10 cm. in length, and weighing about 400 mg., a working sensibility of one millionth part of a milligram was realised with a load of 20 mg. (*i.e.*, a relative sensibility of one in twenty millions). In one series of measurements a maximum sensibility four times higher was realised, but exact weighings were then made very difficult by the tremors in the Högskola building. These tremors, due to the heavy traffic outside, are rather disconcerting, but did not sensibly affect the balance at moderate values of its sensibility.

With the newest type of fibre-suspended micro-balance of reduced size, having a beam only 5 cm. in length, maximum load between 100 and 200 mg., a maximum working sensibility of

one ten-millionth part of a milligram is possible, a limit which is desirable for certain investigations.

On the other hand, fibre-suspended balances of a stronger type have also been made, which could weigh 2 grammes to less than 0·001 mg. Instruments of this type ought to prove useful for micro-chemical work.

The technique of the weighings with the fibre-suspended balances is similar to that employed with the knife-edge balances, although the operations are considerably simplified owing to improvements in the balance case.

The weighings by pressure are carried out with a bulb of known capacity, V, the change in its effective weight due to variation in the pressure and absolute temperature of the air in the case from p_1, T_1, to p_2, T_2 being found from Steele and Grant's equation,

$$dw = \delta_0 \frac{273}{760}\left(\frac{p_1}{T_1} - \frac{p_2}{T_2}\right) . V ;$$

normal density, δ_0 of the air being secured by filtering it slowly into the case through tubes containing soda-lime, barium monoxide and dry cotton wool (the last to remove dust particles). The pressure is read on an ordinary mercury manometer in communication with the case ; the temperature by a thermometer inserted into a special cylindrical tube sunk into the cover. A quarter of an hour is generally sufficient for the case to take up a uniform temperature.

When changes in weight outside the range of the air-weight are to be measured, one has to make use of silica weights standardised "against air." The lightest of those used by Ramsay & Gray weighed 0·1 mg., and were very difficult to handle. It is preferable to make instead "difference weights" for the fractions of a milligram, *i.e.*, a set of small weights of about 1 mg. each, but differing from a certain zero-piece of each set by differences of approximately 1, 2, 2 and 5-tenths, which differences are also standardised against air.

When working in a vacuum or at a constant pressure small changes in weight can be measured by deflection, whereas larger variations may be counter-balanced by a "magnetic weight" consisting of a small steel needle enclosed in silica, depending from the balance and pulled down by an adjustable magnetic field ; this field being produced by sending an electric current through a coil surrounding the weight-tube, and the strength of the current read on a milliampere-meter. The

value of the "magnetic weight" for different strengths of current has, of course, also to be standardised against air.

The theory of a balance suspended by fibres affords several points of interest. It is easy to prove that when the beam swings an extremely short length of the fibres becomes curved. This vital part of the fibre needs to be very thin, generally between 1·5 μ and 2·0 μ for high-sensitivity instruments. The thickness of the rest of the fibres is unimportant. Thin as these fibres are they offer a definite, although very slight, elastic resistance to the bending forces; this increases with increasing deflection of the beam from the upright position, when the fibres are straight. This position consequently represents a *maximum* of the sensibility, which is reduced on either side of it with growing deflection. This variation, which is relatively small at moderate values of the sensibility, is of little importance as regards the technique of the weighings, since the balance is ordinarily worked at constant deflection. Moreover, as it is perfectly constant, the sensibility curve can easily be determined by means of the air-weight.

From a theoretical point of view these variations of sensibility are rather interesting, and have been made the subject of a separate study. Their amount decreases with the radius of the fibres as R^2, whereas the tensile strength of silica fibres is known to decline at a much slower rate; that is, the fibres gain much more rapidly in flexibility than they lose in carrying capacity. One may say, therefore, that the more the size of the instrument is reduced, and the finer the fibres are drawn, the *better* will this kind of suspension work, whereas the opposite is, of course, the case when knife-edges are used.

The fibre suspension presents several other practical advantages over the knife-edge. The latter is apt to get blunt with use and is easily dismounted; it often gets fouled by dust particles. The fibres retain their elastic properties indefinitely and cannot be fouled in any way.

At ordinary pressures the swing of the balance is strongly damped by air friction, but in certain experiments in a charcoal vacuum the beam was found to keep up its swinging motion without any sensible damping, the energy dissipated within the thin fibres being of course infinitesimal. Owing to the perfect properties of this suspension it should be possible to increase the accuracy of the weighings simply by reading the position of the balance very sharply, say with a microscope or

by making use of increased scale distance. The sensibility curve at the end of this Paper seems to confirm this view.

Investigations with the Micro-Balance.

Of the numerous subjects of research with the micro-balance which have suggested themselves to the author, only a few have hitherto been carried out, mainly owing to the pressure of other work.

By weighing in a charcoal vacuum a small sphere of solid silica depending from the balance into a long silica tube, which could be heated up to 1,000° C. by a cylindrical electric furnace, it has been possible to repeat on a diminutive scale and at much higher temperature the experiments of Poynting & Phillips on the influence of temperature upon weight. The upper limit for any possible temperature coefficient of gravity (10^{-9} per 1°C.) found by these authors within the range −180° C. to 100° C., was found to hold up to 460°C. with rather a higher degree of accuracy. When the experiments were repeated at 600°C., the silica sphere was found to undergo a gradual loss in weight at an approximately constant rate, which was much increased when the temperature was raised to nearly 800°. This rather unexpected result indicates that in a high vacuum silica is sensibly volatile even at 1,000°C. below its melting point! This substance cannot therefore be used for an extension of these experiments to still higher temperatures.

A similar or slightly modified experimental arrangement can obviously be used for investigations regarding the volatility of various substances at relatively low temperatures, and also for exact measurements of their vapour tension.

The changes in weight of a piece of gold subsequent to heating, which Ramsay and Gray had discovered and explained as due to adsorbed air, has also been studied. For this purpose a minute electric furnace (made from platinum leaf over a framework of silica) was mounted *within* the weight-tube, so that a piece of gold foil surrounded by the furnace could be heated at different pressures while suspended from the balance, and weighed immediately afterwards. The results proved that an adsorption and liberation of *air* from the gold could not be the cause of the phenomenon, but that this was due to a condensation on the metallic surface of vapours from the rubber grease used to make the case air-tight. Although the effect thus loses some of its theoretical

interest, it is of a certain importance for the technique of the weighings, and will be made the subject of further research.

A subject which has hitherto been practically inaccessible to experimental work owing to the lack of sufficiently sensitive balances is the magnetic susceptibility of diamagnetic gases and vapours. A silica micro-balance affords the most favourable possibilities for such measurements, and it has also been used for determining the magnetic susceptibility of pure nitrogen and hydrogen gas. The former substance does not

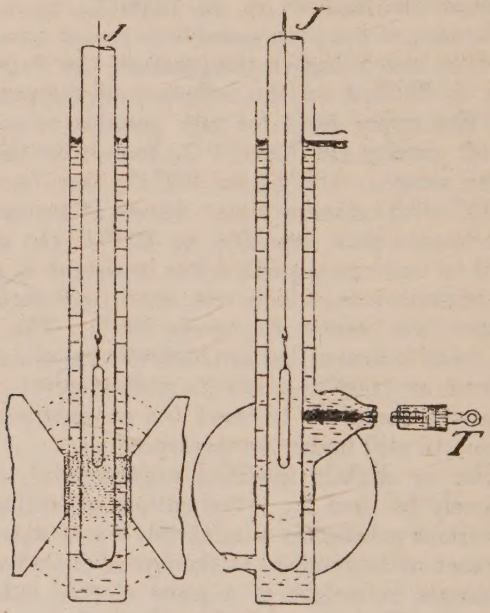


FIG. 3.

seem to have been at all studied by direct experiments, whereas the most recent measurement with pure hydrogen (made by Bernstein in 1909) gave a result about 100 per cent. higher than the correct value.*

The method employed is made clear by the sketch in figure 3, which represents a very light and narrow cylindrical bulb depending from the balance, with its lower end near the centre of a strongly divergent magnetic field.

* Bernstein: "Inaugural dissertation."

The "magnetic buoyancy" due to the gas round the bulb when the field was applied was compensated by varying the pressure. Experiments were made with air (at different pressures) and with very pure nitrogen and hydrogen gas, so that the magnetic susceptibility of the latter could be calculated from the known susceptibility of air. The results agree well with the theoretical values calculated by M. Pascal from the diamagnetism of nitrogen and hydrogen compounds, viz.—*

hydrogen $\kappa = -2{\cdot}62 \times 10^{-10}$ (theoretical value $\kappa = -2{\cdot}6 \times 10^{-10}$)
nitrogen $\kappa = -5{\cdot}57 \times 10^{-10}$ (theoretical value $\kappa = -5{\cdot}0 \times 10^{-10}$)

The susceptibility of liquid hydrogen has been measured by Kamerlingh-Onnes & Perrier, the result recalculated for hydrogen gas giving $\kappa = -2{\cdot}4 \times 10^{-10}$.†

The sensibility of the balance in itself, although moderate, would have allowed of an accuracy of about 1 per cent. That the probable error in the above results is nearly ten times larger is chiefly due to the large errors caused by temperature variations. In order to compensate for the diamagnetism of the silica in the bulb the latter had been filled with a suitable mixture of air and oxygen, so as to make it magnetically neutral at room temperature. The paramagnetism of the enclosed oxygen being a function of the temperature, this arrangement was highly susceptible to temperature changes, which could not be entirely eliminated by the water-jacket shown in the sketch. With a similar experimental arrangement modified so as to neutralise the temperature effect, the magnetic properties of other gases and vapours might be accurately measured. The author hopes especially to investigate whether when absorbing or fluorescing under the influence of light a gas or vapour has its magnetic susceptibility affected.

There are, besides the subjects mentioned, a great number of others calling for investigation with the micro-balance. Only a few of these will be here indicated.

The experiments of Nichols & Hull‡ on the pressure of light might be repeated with a much simpler arrangement, by weighing on a highly sensitive micro-balance the pressure exerted on a reflecting disc from below by an ascending beam of light. An accuracy of measurement of 1 per cent. or even

* Pascal: "Ann. Chim. Phys.," VIII., 19, p. 5.
† Kammerlingh-Onnes and Perrier: "Comm. Phys. Lab. Leyden," 122A., p. 10.
‡ Nichols & Hull: "Drude's Ann.," 12, p. 225.

less would be quite feasible, and would make possible interesting variations in the conditions of the experiment.

The ingenious method due to Prof. Knudsen* for measuring accurately very low pressures by an "absolute manometer" (by measuring the radiometric force between two discs of known unequal temperatures in a gas at low pressure) might be applied to the measurement of absolute temperatures with the aid of the micro-balance. If two horizontal discs of unequal temperatures T_1 and T_2, one of them suspended from the balance, are brought within suitable distance of each other, the radiometric force F between them could easily be determined by weighing. Supposing the pressure p of the gas in the case (say highly rarefied helium) to be accurately known, then the ratio between the absolute temperatures is given by Knudsen's equation $F=\frac{p}{2}\sqrt{\frac{T_1-T_2}{T_2}}$, and if either temperature is known the other can be calculated. By this method temperatures near absolute zero might be measured with a high degree of accuracy.

According to modern conceptions of the relationship between energy and matter the gain or loss of a certain amount of energy, dE, from a body should correspond to a change in its weight, dW, given by $dW=\frac{dE}{c^2}$, where c is the velocity of light. All ordinary changes in energy are too small to give rise to any perceptible changes in weight according to this formula, the only exception being those accompanying radioactive disintegration. If a large quantity of radium emanation, say, 2 curies, could be weighed, first fresh and then after disintegration into Radium D, the "weight" of the energy given off in the meanwhile should be of the order of one millionth of a milligramme, an amount which might be measured with a highly sensitive micro-balance. Owing to the intense thermal and electric phenomena accompanying the disintegration, the technical difficulties of making such weighings will no doubt be formidable, but the importance of such an "experimentum crucis" appears so considerable as to make it well worth an attempt.

The Strömberg Suspension.

The number of investigations open to micro-balances, which would add at least three or four decimals to the ordinary range

* Knudsen: "Ann. der Phys.," 28, p. 75.

of measurement, with a gain instead of a loss in relative sensibility, is obviously almost unlimited.

It is therefore rather remarkable that, except for the work of Ramsay and Gray, the quartz micro-balance remains almost unused even to-day. The reason is probably that it appears to be regarded as extremely difficult to work with, and requiring a quite unusual manipulative skill on the part of the operator. This impression had some foundation as regards the knife-edge balances and also, although to a less degree, regarding the early types of the fibre-suspended balances. Thanks to some very important improvements in construction

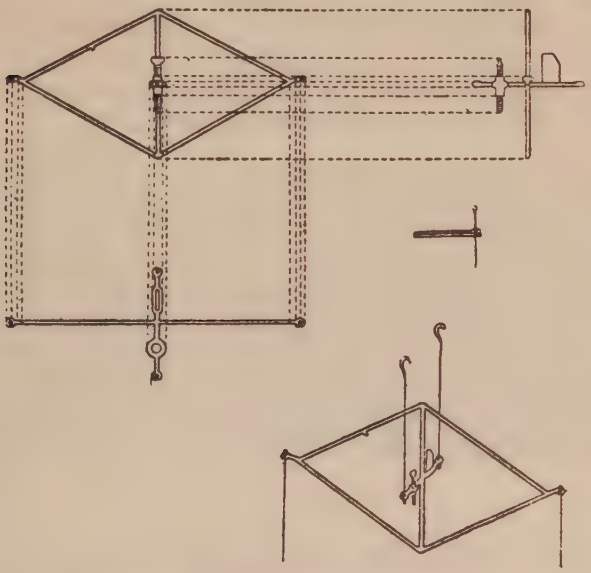


Fig. 4.—The Strömberg Suspension.

which have lately been worked out at Stockholms Högskola, by Dr. Strömberg, these are now quite as simple to work with as any other highly sensitive physical instrument.

The Strömberg suspension is sketched in Fig. 4, and needs but a few words of explanation. The fibres are made detachable, with small conical lumps at one end which fit into small silica rings on the beam. The fine part of the fibre is passed through a narrow cut in the side of the ring, and the conical lump is secured in its place with a minute quantity of shellac. If a fibre should be accidentally broken a reserve fibre can be

inserted in its place in a few minutes without removing the balance from its place.

In the early instruments where the fibres were drawn out from the beam, a broken central fibre could only be repaired by dismounting and inverting the beam and drawing out from it a new fibre, an operation which was rather delicate and not without a certain risk of destroying one of the other fibres. Another improvement of considerable practical value, which has also been worked out by Dr. Strömberg, is the tiny magnalium screw seen in the figure, by which the centre of gravity of the beam (and consequently also its sensibility) can be raised without difficulty. The older method of sealing on to the beam, or drawing off from it minute pieces of silica, was dangerous to the fibres, and also tedious as it was done quite at random, and therefore had to be repeated a great number of times in order to realise the desired adjustment.

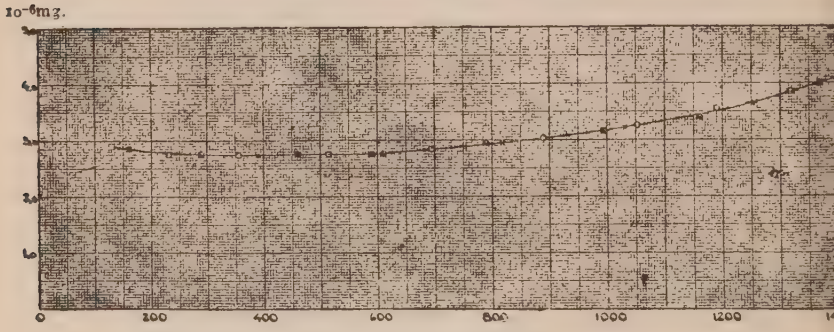


Fig. 5.

With the permission of Dr. Strömberg I am able to reproduce in Fig. 5 a sensibility curve obtained with one of his instruments (which I had the pleasure of showing to the Physical Society of London at its meeting on March 12, 1920). It is derived from three independent series of measurements, and although the scale-sensibilty is very moderate ($2{\cdot}7 \times 10^{-6}$mg. per 1 mm. scale-deflection at four metres scale-distance), corresponding to a relatively short period of swing, the different points on the curve are seen to agree to within less than one ten-millionth part of a mg. This result lends support to the view that the suspension of the instrument is in itself sufficiently delicate to allow the working sensibility to be considerably raised, merely by increasing the accuracy with

which the position of the balance is observed, without any further prolongation of the period of swing.

Thanks to these improvements made by Dr. Strömberg, who will shortly publish an account of his own work, the technique of the fibre-suspended micro-balance has been much simplified, and it can now be used by anybody with scientific training. Without exaggeration one may, therefore, say that this micro-balance, which in its origin is due to the genius of British men of science, to Mr. C. V. Boys, who first invented the wonderful quartz-fibres, to Steele and Grant, to Ramsay and Gray, has by stages been developed into a working instrument which provides the chemical and physical sciences with a new and powerful weapon for future research.

ABSTRACT.

Previous Micro-balances.—Brief reference is made to the instruments of Angström, Salvioni, Nernst, and especially to the all-quartz balance of Steele and Grant.

The Author's Micro-balance.—The instrument devised by the author, and here described, is also wholly of quartz. The knife-edge support is, however, replaced by a suspension consisting of a pair of quartz fibres. A magnetic arrestment is employed. Balance is obtained either (*a*) by an "air-weight," the upward displacement on a bulb being varied by varying the gas pressure within the enclosing case; or (*b*) by a "magnetic weight," involving the use of a measured electric current. The range of weighings depends on the size of beam: with one 5 cm. in length the maximum load is between 100 and 200 mg., and the setting can be made to within 10^{-7} mg.

The Strömberg Suspension.—Improvements in the construction devised by Dr. Strömberg are described, which obviate the formidable difficulties of manipulation that have hitherto discouraged workers from employing the instrument.

Investigations with the fibre-suspended Micro-balance.—A brief description is given of the following researches already carried out:—(1) The influence of temperature upon weight. This is an an extension of the work of Poynting and Phillips on this subject; (2) the volatility of silica; and (3) the susceptibility of diamagnetic bodies.

It is proposed to conduct investigations on the pressure of light, on the measurement of absolute temperature (based on an equation of Knudsen's), and on the loss of mass accompanying loss of energy.

XXII. *A Modified Theory of the Crookes Radiometer.* By Gilbert D. West, M.Sc. (Lond.).

A striking feature of the physical literature of the period 1874-1881 is the extraordinary amount of interest that was raised by Crookes' researches "On the Repulsion Resulting from Radiation." In addition to Crookes himself, Fitzgerald, Johnstone-Stoney, Stokes, Schuster, Pringsheim, Osborne-Reynolds, and Maxwell, all gave the phenomena their attention. It was the work of Osborne-Reynolds and Maxwell, however, that was held to settle the numerous discussions that had arisen. These physicists showed, in two long and highly mathematical Papers, published in the Phil. Trans. of 1879, that the phenomena under notice were explicable in terms of the then somewhat new kinetic theory of gases. As a result of their Papers, the interest in the theory of the radiometer—great though it had previously been—subsided, and but for a criticism of Reynolds' work from Fitzgerald in 1881,* there is little worth recording for the next sixteen years. It would seem that this change occurred, largely because the majority of physicists felt that the matter had been placed beyond their grasp, rather than because some well-understood explanation had been given which rendered further research superfluous. According to Fitzgerald, Reynolds had rendered "a difficult subject tenfold as elaborate as was necessary," but however this may be, it is certain that an easily intelligible explanation of radiometric phenomena was wanted. Attempts were made, it is true, to give the substance of the accepted theories in general language, but, although many of the text book "explanations" are still based on these, it cannot be said that they met with much success.

In 1896, however, Sutherland turned his attention to the subject, and his Paper† had the advantage that, unlike those of Reynolds and Maxwell, it could be read with profit by the physicist of ordinary mathematical attainments. Yet appearing as it did at a time when the interest in the theory of the radiometer had practically ceased, it was neglected, and has since become almost entirely forgotten.

It was shown by the author,‡ however, that Sutherland's theory of the closely related phenomena of thermal transpira-

* Phil. Mag., 11, p. 103, 1881.
† Phil. Mag., 42, p. 373 and p. 476, 1896.
‡ Proc. Phys. Soc., Vol. XXXI., p. 278, 1919.

tion was so far correct, that it could be made to form an adequate basis for the careful experimental work of the Danish physicist, Knudsen, performed thirteen years later. The author was thus tempted to place considerable confidence in Sutherland's theory of the radiometer. It must be observed, however, that, at the time when Sutherland wrote his paper, little was known of the thermal surface conditions in a rarefied gas, and thus it is not surprising to find that Sutherland's theory —like many another imperfect theory in other branches of physics—will explain some of, but not all the experimental facts. It is the object of the present Paper to outline a theory of a general descriptive character which, while being to a large extent based on Sutherland's work, makes use of knowledge gained since that time.

Sutherland, like Reynolds, bases his theory of the radiometer on the closely related phenomena of thermal transpiration. He first considers the case of a tube along which a temperature gradient is maintained. If the tube connects two infinite spaces, he shows that an even flow of gas takes place over the whole cross section of the tube from the hot to the cold side. The walls of the tube in thus constraining the gas to take their temperature, exert a tangential traction upon it, whilst they themselves experience a reaction in the opposite direction. A nett reaction is only experienced, however, near the entrance of the tube, where the velocity of the gas rises from zero to the uniform value eventually attained. At all other parts of the tube the friction of the moving gas against the walls of the tube exactly balances the traction the walls exert on the gas ; it is to the " unequilibrated traction," however, that Sutherland attaches most importance.

If the spaces the tube connects are finite, the uniform flow of gas produces a pressure in the hot vessel, which in turn produces a flow of the Poiseuille type in the reverse direction to the original flow. The result of the superposition of these two flows is to give a gas current from the cold to the hot side along the walls of the tube, together with a current from the hot to the cold side along the axis, whilst between the two currents there is a surface of zero velocity.

With reduction of gas pressure, the Poiseuille counter flow becomes less and less important, until at the highest rarefactions it is negligible, and the hot regions are then enabled to maintain undiminished their higher pressures.

Sutherland points out that both the "unequilibrated traction" and the thermal transpiration pressure operate in causing radiometer motion, and to make his meaning clear he takes the case of a piston that fits loosely into a cylinder closed at both ends. He imagines the temperature of one compartment to be higher than that of the other, and that a temperature gradient exists in the material of the piston. As a consequence of the operation of thermal transpiration, the pressure on the hot side will become greater than that on the cold side, and a force will be exerted on the piston, both as a result of this excess pressure in the hot compartment, and as a result of the unequilibrated traction on the sides of the piston. By increasing the clearance between the piston and the cylinder, the thermal transpiration pressure can be made to bear an ever-decreasing ratio to the total thrust on the piston. If the piston be free to move, it constitutes, according to Sutherland, an exaggerated example of radiometer motion, and he formulates his theory on these lines.

It must be admitted, however, that to pass from the case of the piston to that of the vane of a Crookes radiometer, is a big step. Nevertheless, Sutherland maintains that such a step is legitimate and proceeds, with considerable success, to analyse the experimental data obtained with a type of torsion radiometer. Except in so far as it was better suited to give quantitative measurements, this type did not differ materially from the familiar type in which flat circular vanes, blackened on one side and silvered on the other, are mounted at the ends of a pivoted arm. "When the black face is irradiated," he says, "there is a fall of temperature . . . through the thickness of the vane, and thus the thickness of the vane becomes a surface capable, along with the surface of the bulb opposite it, of starting thermal transpiration from the cold edge to the hot, with elevation of pressure in front of the hot face . . . and depression of that behind the cold face."

Such, briefly, is an outline of Sutherland's theory, but it is clear, as indeed he himself states, that it is based on the idea that the effect of the introduction of a heated body into a gas is "to make the layer of gas in contact with the solid take the temperature of the solid at every point of the surface." It has since been realised, of course that this is not so, and that in any calculation made in regard to rarefied gases, due

allowance must be made for the temperature discontinuity at the surface of the solid.

To make what is meant more clear, consider the case of two large parallel plates—one hot and the other cold. We may regard the molecules that strike the hot surface, both as coming from a distance of the order of the mean free path, and as possessing the temperature of this region. If, after reflection at the surface, these molecules merely acquire its temperature, it is clear that the mean temperature of the surface layer of gas is necessarily lower than that of the surface itself. As a matter of fact, however, it has been shown* that, on the average, impinging molecules do not even acquire the temperature of the surface they strike, and hence the discontinuity already noted becomes enhanced.

Thus in the light of modern research, one of Sutherland's assumptions is seen to be unjustifiable, and the theory built upon it must therefore be imperfect. It is possible, however, to modify the theory.

Consider once again the case of the hot and cold plates, but now with reference to the change that takes place in the isothermals, as the pressure is lowered. It is clear that, with increasing mean free path, the extreme isothermals will disappear, and that eventually, when the mean free path becomes large compared to the distance apart of the plates, the gas between will reach a uniform mean temperature. Reduction of gas pressure will thus be accompanied by a gradual diminution, and eventual elimination, of the temperature gradient between the plates.

In the particular case considered, it is easy to calculate the positions of the isotherms with changing gas pressure, but in the case of a radiometer vane this is much more difficult. However the present purpose is served quite well if a general indication is given of the changes that take place.

To make matters simple, consider first of all the case of a disc with the hot surface A 10° C. above the cold surface B—the walls of the containing vessel being supposed distant, and at the mean of the temperatures of the hot and cold surfaces. In Fig. 1 the isotherms in the gas are drawn for the case where the gas pressure is very high, whilst in Fig. 2 a

* Soddy & Berry, Proc. Royal Soc., A, 84, p. 576, 1911; Knudsen, "Ann. d. Phys.," 34, 4, p. 593, 1911; Smoluchowski, "Phil. Mag.," XXI., p. 11, 1911; Langmuir, "Phys. Rev.," Vol. II., No. 5, p. 329, 1913.

rough idea is given of their appearance at a lower pressure. It will be seen that the extreme isothermals have disappeared, and that, so far as the gas is concerned, neither surface of the disc coincides with an isothermal surface. On the contrary, a temperature gradient extends from the circumference towards the centre.

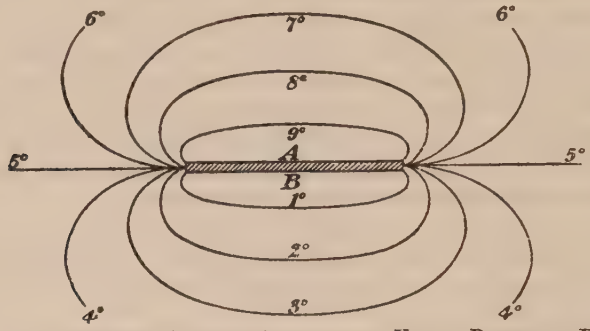


FIG. 1.—ISOTHERMS AROUND RADIOMETER VANE—PRESSURE HIGH.

Such a temperature gradient in the surface layer of gas was encountered in the experiments described in a previous paper by the author on the forces acting on heated metal foil surfaces in rarefied gases,* and it was there shown that satis-

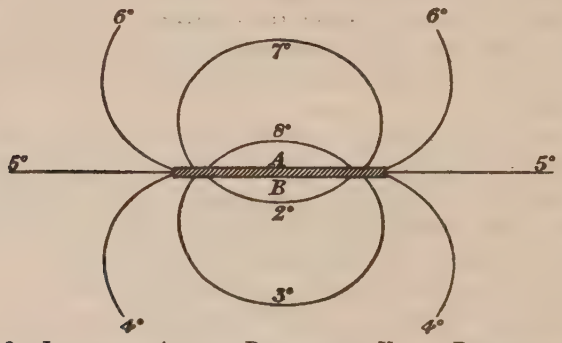


FIG. 2.—ISOTHERMS AROUND RADIOMETER VANE—PRESSURE LOW.

factory explanations could be based on the assumption that this temperature gradient gave rise to gas currents somewhat similar to those arising in a tube in the material of which a temperature gradient was maintained in the direction of the length.

* "Proc." Phys. Soc., Vol. XXXII., 1920.

If, as would appear legitimate, we can apply similar reasoning to a radiometer vane, we shall have to imagine a flow of gas to take place similar to that indicated in Fig. 3. On the cold side B, the gas will be flowing outwards from the centre, with consequent reduction of pressure on the vane, whilst on

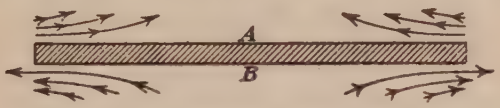


FIG. 3.—FLOW AROUND RADIOMETER VANE.
A is Hot Surface, *B* is Cold Surface.

the hot side A it will be flowing inwards towards the centre, with consequent increase of pressure on the vane. When the gas pressure is fairly high, and when the isotherms have consequently not departed much from the shapes indicated in Fig. 1, the flow of gas and the regions of excess and defect of pressure will be restricted to the peripheral portions of the disc. When,

FIG. 4.—ISOTHERMS AROUND RADIOMETER VANE IN SPHERICAL GLASS BULB—MEDIUM GAS PRESSURE.*
A is Hot Surface, *B* is Cold Surface.

however, the gas pressure is lowered, the area over which the excess or defect of pressure is felt, will extend towards the centre, until at last the whole surface of the disc will be covered.

The shapes of the isotherms surrounding an actual radiometer vane enclosed in a spherical bulb are, of course, again

* The suggestions made by Mr. F. J. W. Whipple in the discussion are embodied in this figure.

far too complicated to calculate. From general considerations, however, it is possible to give a rough idea of their shapes. In practice, one side of the vane, although much cooler than the other, is still slightly above the temperature of the glass walls, and under such conditions Fig. 4 might represent sufficiently well the forms the isothermals would take at a medium gas pressure. An indication is also given in Fig. 5 of the directions in which the gas currents would flow. It will be seen that on both sides of the vane they flow towards the central region, but that on the hot side the flow is much more vigorous owing to the greater temperature gradient. It will be noticed further that the flow is greatest on the edge of the disc nearest the

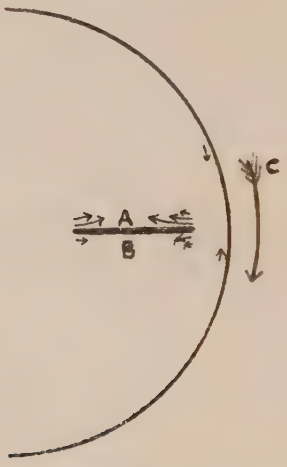


Fig. 5.—Flow Around Radiometer Vane in Spherical Glass Bulb. *A* is Hot Surface, *B* is Cold Surface.

glass, and also that in the gas layer on the glass itself there are two regions each capable of producing excess pressures—the most effective region being near the hot side of the disc.

Briefly, therefore, the modification introduced by the present explanation consists chiefly in a difference in the temperature ascribed to the surface layer of gas covering the vane, and in a consequent difference in the regions over which the flow of gas is supposed to take place. The question naturally arises, therefore, as to whether there are any radiometric phenomena that support the present view. For a reply we must turn our attention to the Crookes radiometer with slanting vanes.

In this instrument, a plan of which is shown in Fig. 6, a number of vanes are placed at an angle to the supporting radial arms. In one instrument the vanes were made of aluminium foil bright on both sides, and when radiation was allowed to fall upon them, vigorous rotation ensued (indicated in Fig. 6 by the arrow), and that in spite of an almost negligible difference in temperature of the two sides of the vanes. Crookes points out * that had the vanes pointed radially there would have been practically no tendency to rotation.

On Sutherland's theory, no very obvious explanation of this phenomenon can be given, but on the present theory this is at once possible.

The surfaces of the vanes will be covered on both sides by a gas layer whose temperature will rise from the edge towards

FIG. 6.—CROOKES RADIOMETER WITH SLANTING VANES.

the central regions. The gradient will be steepest on the part of the disc nearest the bulb, and here also the flow of gas will be most vigorous. We have to remember however that the gas currents can only flow as a result of a tangential reaction exerted on the material of the vane. Hence on both its surfaces we shall have outward tangential forces, and such forces will be capable of producing rotation. The side of the vane nearest the glass, moreover, will experience a pressure resulting from the difficulty the surface currents find in escaping, but this pressure will not in general be as important as the tangential forces.

The radiometer with slanting vanes connects itself naturally with the recent experiments of the author† on the edgewise movements of strips of foil placed at an angle to a glass

* Phil. Trans., 169, p. 282, 1878.
† Proc. Phys. Soc., Vol. XXXII., 1920.

wall, and exposed to the radiation from a lamp. It will be remembered that such strips showed edgewise movements towards the glass which completely masked the normal repulsion effect.

Let us now pass to the consideration of the radiometer constructed of hemispherical cups, mounted anemometer fashion. Sutherland says: "Consider a convex vane irradiated by a source on the normal through its middle point; then as the amount of heat that a surface absorbs depends on its obliquity to the incident radiation, the farther a part of the convex surface is from the middle, the less is it directly heated, and thus there is a continuous fall of temperature from the centre of the surface to the edge; conduction, if allowed time, tends to reduce the amount of fall but does not obliterate it, and conduction also establishes a fall of temperature along the back from the centre to the edge. Now the traction of the gas on the solid, is from hot to cold, so that both on the front and back of the vane there is a traction from centre to edge, whose resultant effect is to drag the vane away from the light when the vane is convex to it, so that the light appears to repel a convex surface. When the surface is concave the same reasoning applies, the gas exerts a traction from centre to edge, and therefore the light appears to attract it."

According to the above, therefore, the temperature gradient is made to depend on the changing obliquity of the surface of the cup. Rotation still takes place, however, when the obliquity is kept constant by replacing the hemispheres by hollow cones. It is true that, owing to a spreading of the stream lines more heat will here be conducted away from the peripheral regions of the cone than from other regions, but with cups of good conducting material, such as aluminium, the temperature gradient so produced would be very small. Previous experiments by the author have demonstrated, moreover, that such temperature gradients are not of much importance. For the rotation of the conical cups, therefore, Sutherland's theory gives no easy explanation.

If we realise, however, that a rarefied gas does not necessarily take the temperature of the surface with which it is in contact, and if we imagine, as is legitimate, that a temperature gradient, in the gas layer, extends from the periphery of the cone towards the apex, there is no difficulty in modifying Sutherland's original explanation to suit this new case. Such

a modification will resemble that given for the radiometer with slanting vanes.

In his Paper Sutherland deals with other experiments performed by Crookes, but the differences that have to be introduced into his explanations on the present theory are so small that further discussion is not necessary.

It might be said, therefore, when all the facts that have been discussed are taken into consideration, that Sutherland's theory, as it stands, is apparently not capable of explaining certain radiometric phenomena, but that when modifications, supported by modern experimental knowledge, are made in the thermal surface conditions, such explanations become at once possible.

ABSTRACT.

The Paper gives a short account of a theory of the Crookes Radiometer worked out by Sutherland in 1896, but unfortunately since much neglected. The theory as it stands will not explain many radiometric phenomena, but it is shown that when modifications, depending on the modern knowledge of thermal surface conditions, are made, such explanations become possible.

Radiometer action, especially at the higher gas pressures, would appear to depend essentially on the formation of gas currents near the radiometer vane. These currents are distinct from convection currents, but are closely connected with the phenomena of thermal transpiration.

The present work is the natural outcome of previous work by the author (Proc. Phys. Soc., Vol. XXXII.).

DISCUSSION.

Mr. F. J. W. WHIPPLE pointed out that as the isothermals were drawn in Fig. 4, they indicated that the colder side of the vane was actually colder than the walls of the bulb, whereas actually it was hotter. He indicated alterations in the shape of the isothermals which would correct this. In connection with the general presentation of the problem he would have liked it better if it had been based more on kinetic theory and less on hydrodynamical flow. It was necessary to introduce kinetic theory to explain the difference of pressure between the hot and cold ends of a tube. When the mean free path was large, a particle entering one end of the tube would strike the sides tangentially and pass through the tube. When equilibrium was reached the number of particles entering at each end must be equal, so that $\rho v = \rho' v'$. But the condition for equality of pressure was $p v^2 = \rho' v'^2$.

Mr. WEST, in reply, said he was not quite clear at the moment on the point raised about Fig. 4. He would look into it and communicate a reply.* In an earlier Paper he had pointed out what Mr. Whipple had just mentioned, that at low pressures it was the conservation of matter that determined equilibrium, while at high pressures it was the equality of pressures.

* See footnote to page 227.

XXIII. *On the Magnetic Properties of Silicon Iron (Stalloy) in Alternating Magnetic Fields of Low Value.* By Albert Campbell, *B.A.* (*From the National Physical Laboratory.*)

Received February 28, 1920.

1. *Introduction.*

The behaviour of magnetic materials, and in particular of silicon iron, at very low magnetisations, is of importance in telephony and radio telegraphy, as telephones and transformers in general have iron cores. The following investigation was carried out in 1916 and 1917 for the Board of Invention and Research, and is now published by the kind permission of the Admiralty. The particular silicon iron tested was Stalloy made by Messrs. J. Sankey & Son.

As the hysteresis loss, with which we are here chiefly concerned, falls off more rapidly than the square of the flux-density **B**, it becomes so small at low magnetizations that it is difficult to measure it with accuracy. In Prof. E. Wilson's experiments,* to which we shall allude further below, the lowest value of **H** used was 0·0036. We succeeded in making measurements down to about $\mathbf{H}_{max.}=0{\cdot}0002$. For this low value the total amount of power measured was of the order of one millionth of a microwatt. In addition to determinations of power loss both at low and telephonic (audio) frequencies, the difference in properties of wire and sheet samples was examined, and investigation was made of the effect of superimposing on the small alternating field a much larger direct-current steady field.

2. *Hysteresis Loss at Low Frequencies.*

The silicon iron in the principal tests was in the form of ring stampings of Messrs. Sankey's stalloy sheet about 0·4 mm. thick, insulated on one side. A number of these had been made up into a ring of mean diameter 7 cm. and section 3 sq. cm., with 10 windings of 10 turns each and 4 of 200 turns each, being, in fact, one of our small laboratory transformers.

With 10 primary turns and 800 secondary, various ballistic tests were made, but the sensitivity was not sufficient below values of **H** of about 0·02. Accordingly I had recourse to an

* Proc. Roy. Soc., p. 548, Vol. 80, June, 1908.

alternating-current method which is very much more sensitive, and had been found very convenient for similar measurements of the power lost in the iron wrappings of loaded telephone cables. I have already described the method (Proc. Phys. Soc., p. 24, Vol. 22, 1910), but a brief description here will make matters clearer.

The iron ring is wound with superimposed primary and secondary turns N_1 and N_2 respectively, m being the mutual inductance between them. They are connected, as shown in Fig. 1, through the coils of a mutual inductometer D to a

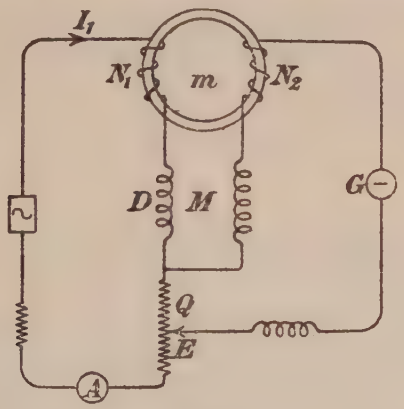


FIG. 1.

source of alternating current and a vibration galvanometer (G), a sliding contact E allowing a portion Q to be selected from a slide-wire or other suitable resistance connected to the junction of the coils of D. Let the primary current I_1 (by tuning or otherwise) be of sine wave form, and let the galvanometer be tuned to its frequency. The galvanometer deflection can now be reduced to zero by adjusting D and E to values M and Q respectively. When the eddy currents in the ring are small we have

$$m=M,$$

and total iron loss

$$=\frac{N_1}{N_2}.QI_1^2.$$

Since $m=4\pi\times10^{-9}N_1N_2\mu s/l$, where μ is the permeability, s the section and l the mean circumference of the ring, we can at once find the permeability μ for various values of I_1 (and hence

of **H**). The value of **H** is deduced from $I_1\sqrt{2}$, where I_1 is the effective value of the sine wave current (measured by the ammeter A).

The low-frequency tests were made chiefly at 10 and 75 ∿ per second. At 10 ∿ per second the oscillations of the light spot from the vibration galvanometer are easily visible, and extreme accuracy of reading can be obtained by adjusting until no tremor is perceptible.

With **H**=0·004 at 10 ∿ per second, M (and hence μ) could be read with a sensitivity of 1 in 10,000, while the loss could be observed to 1 or 2 per cent. At 75 ∿ per second the eddy current loss forms a considerable fraction of the total power loss, being more than half the total at values of **H** below 0·0004. For both frequencies the eddy current losses were calculated from the formula

$$h' \doteqdot 1{\cdot}64\,\frac{(2a)^2 n^2 \mathbf{B}^2_{\max.}}{\sigma},$$

where h' is in ergs per cubic centimetre per cycle, σ is the resistivity (here about 57,000), $2a$ the thickness of the sheet, and n the frequency.

For the upper values of **H** (about 0·01) the hysteresis loss thus derived is in good agreement for the two frequencies. At the lower range of **H** there is some divergence, the higher frequency giving the smaller values. In any case the eddy current loss is of small importance at 10 ∿ per second, and the result which we proceed to give has therefore been deduced only from the observations at this frequency.

Bearing in mind that μ remains nearly constant over the whole range of **H** dealt with, we deduce $\mathbf{B}_{\max.}$ from $\mathbf{B}=\mu\mathbf{H}$. If h be the observed hysteresis loss (in ergs per cubic centimetre per cycle), when log h was plotted against log **B**, the resulting curve was found to be nearly a straight line. The results are quite well represented by the two following equations :—

H range.		**B** range.		Equation.
0·0002 to 0·002	.....	0·051 to 0·51	.....	$h \doteqdot 6{\cdot}0\times10^{-6}\,\mathbf{B}^{2\cdot3}$
0·002 to 0·02	.....	0·51 to 5·1	.....	$h \doteqdot 6{\cdot}6\times10^{-6}\,\mathbf{B}^{2\cdot40}$

These results refer only to the particular sample tested, for which the permeability μ over the range of **H** employed is about 254.

Prof. E. Wilson's ballistic tests on stalloy (of initial permeability about 260) for a range of $\mathbf{H}$ from 0·004 to 0·04 give

$$h \doteqdot 6{\cdot}9 \times 10^{-6}\mathbf{B}^{2{\cdot}30}.$$

Our own ballistic tests from $\mathbf{H}=0{\cdot}015$ to 0·1 gave

$$h \doteqdot 14 \times 10^{-6}\mathbf{B}^{2{\cdot}4};$$

but magnetic viscosity effects somewhat complicate tests with slow cycles.

It appears that as $\mathbf{H}$ is lowered the index of $\mathbf{B}$ decreases slightly. I have little doubt that the constants will vary very considerably from sample to sample, but I think that the equations given represent the properties of good material ($\mu_0=254$). It may be mentioned that the values of the permeability obtained at 10 and 75 ∼ per second were almost identical (within 4 parts in 1,000), rising from 254 at $\mathbf{H}=0{\cdot}00008$ to 264·5 at $\mathbf{H}=0{\cdot}01$.

3. *Power Loss at Telephonic Frequencies.*

By the same method, but using a telephone in place of the vibration galvanometer, similar tests were made both at 500 and 800 ∼ per second, the values of the applied $\mathbf{H}_{max.}$ running from 0·0002 up to 0·1. When the logarithm of the total power loss was plotted against log $\mathbf{B}_{max.}$ the resulting curve was in each case an approximate straight line, the line for 500 ∼ per second being parallel to but somewhat below that for 800 ∼ per second. The hysteresis loss was calculated by the same formula as before, assuming uniform flux density in the material. The (log $\mathbf{B}$, log h) curves at the two frequencies come to nearly the same straight line. This line is parallel to, but considerably above, the line given at 10 ∼ per second, showing the effective hysteresis loss at 500 ∼ per second to be about three times that at quite low frequencies. Whether this difference is due to magnetic viscosity, or to want of uniformity of $\mathbf{B}$ throughout the section of the material (skin effect), or to some omitted correction, is not clear; the point requires further elucidation.

In some of the experiments the material appeared to show a slightly lower initial permeability (*i.e.*, for $\mathbf{H}=0$) at the higher frequencies, but this was probably due to effects of previous history which are discussed in §5 below.

4. *Differences between Wire and Sheet Samples.*

With stalloy as supplied commercially we have noticed that various samples in the form of wire do not show nearly such good magnetic properties as the sheet material. This is unfortunate, as wires have advantages for certain types of core. The cause of the difference was investigated, and it appears to be due to insufficient annealing of the wire. Various samples of wire were annealed in hydrogen (or nitrogen) at a tempera-

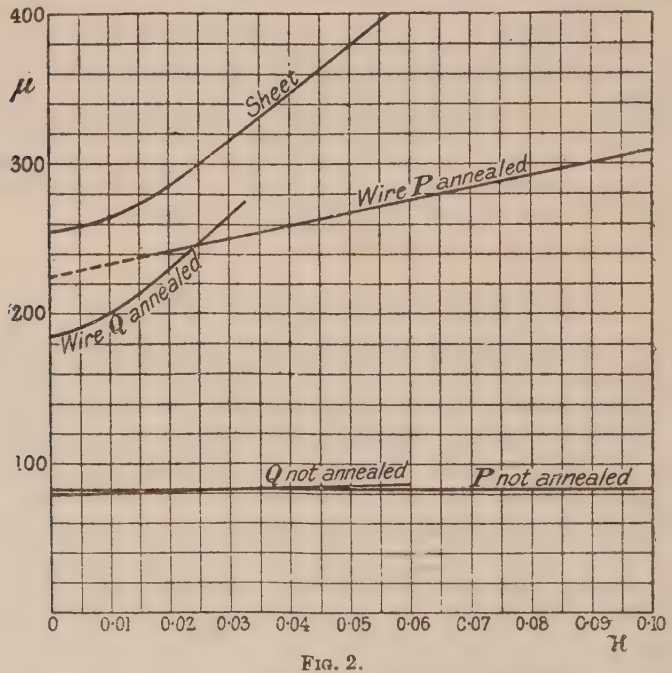


Fig. 2.

ture of about 800 °C. Some were tested in ring form, others in long thin bundles placed (with a central search coil) in the middle of a long magnetizing solenoid. In all cases for the lower values of **H** the mutual inductance method was used. The results for the long bundles were corrected for the demagnetizing field due to the ends. The main effect of the annealing was to increase the permeability; the power losses for the same value of **B** did not show any marked improvement. Some of the results for permeability are shown in Fig. 2. The wires marked *P* and *Q* had diameters of 0·15 mm. and 0·40 mm. respectively.

It will be noticed that the unannealed wires show very constant permeability for small values of **H**. The thicker wire (*Q*) behaves similarly to unannealed Swedish iron as given by Lord Rayleigh, for which $\mu = 81 + 64\mathbf{H}$. The thinner wire (*P*) shows much greater constancy in permeability. The annealing makes a very great improvement in the wires, (*P*) annealed being almost as good as the sheet material. It is interesting to notice that it still retains a slower rate of increase of permeability for change of **H** than is shown by (*Q*) annealed. The wire (*Q*) after annealing is very like the very soft Swedish iron for which Ewing gives $\mu = 183 + 1382\mathbf{H}$.

It is clear that for practical purposes, except where constancy of μ is important, silicon iron wires should be carefully annealed.

5. *Effect of Superimposed Direct-Current Field.*

In many cases in practice a core of magnetic material used with alternating magnetization may at the same time be subjected to a direct-current field; for example, in a telephone transformer used with a microphone receiver.

It is of interest therefore to investigate the effect of such a superimposed field upon the behaviour of the magnetic material as regards the alternating field. For this purpose tests were made on the Stalloy sheet and on the unannealed wire (*P*), both with ballistic galvanometer to represent quite slow frequencies (say, $2\sim$ per second), and also at a frequency of $800\sim$ per second. As a preliminary to these tests the ordinary permeability curve was determined for each of the materials. As will be seen from the μ curves in Fig. 3 for sheet and Fig. 4 for unannealed wire, the sheet material shows much higher permeability throughout than the unannealed wire.

6. *Ballistic Tests with Small Superimposed Cycles.*

In order to examine the effects at quite low frequencies, ballistic tests were made as follows. The material was first demagnetized by repeated cycles of gradually diminishing **H**, and then brought into a steady cyclic state by many reversals of a given $\mathbf{H}_{max.}$. The corresponding $\mathbf{B}_{max.}$ is known from the permeability curve already determined. When in the condition ($\mathbf{H}_{max.}$, $\mathbf{B}_{max.}$), the **H** is suddenly reduced by a small amount $\Delta\mathbf{H}$ by the introduction of a suitable resistance into the magnetising circuit. By cutting this resistance out and in, a small cycle of amplitude $\Delta\mathbf{H}$ is superimposed on the main field $\mathbf{H}_{max.}$.

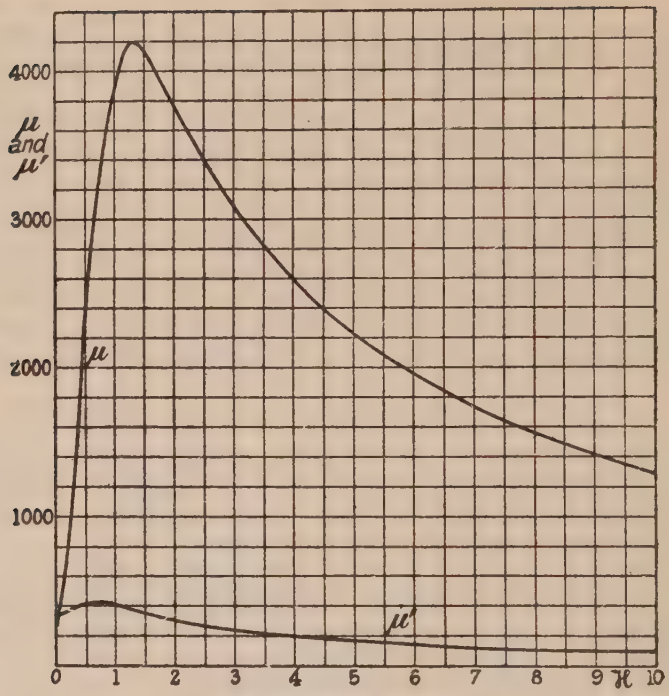


FIG. 3.—SHEET MATERIAL.

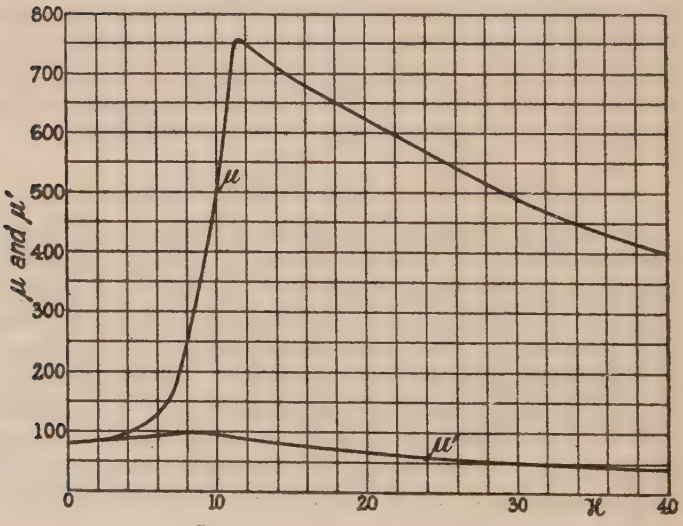


FIG. 4.—UNANNEALED WIRE.

This cycle was repeated 20 or 30 times to approximate to a steady state, and then the $\Delta\mathbf{B}$ corresponding to $\Delta\mathbf{H}$ was determined by the throw on a sensitive ballistic galvanometer.

In general $\Delta\mathbf{H}$ was not more than from 0·1 to 0·02 of $\mathbf{H}_{max.}$. The ratio $\Delta\mathbf{B}/\Delta\mathbf{H}$ was taken as the effective permeability μ' for the small cycle. The values obtained are plotted against $\mathbf{H}$ (the main field) in Figs. 3 and 4. At the lower values of $\mathbf{H}$ (and where the $\mathbf{H}$, μ curve is rising steeply) the previous history of the material has large effect on μ', and the experimental results are a little uncertain. For each material, however, the μ' at the lower values of $\mathbf{H}$ is distinctly approaching the initial permeability μ_0 got by the more direct methods.

An examination of the curves shows that, for small slow cycles, the effective permeability μ' is considerably increased as the main field is raised. But before $\mathbf{H}$ has reached the region of maximum μ, μ' has begun to fall off again, and, as $\mathbf{H}$ goes on increasing, it soon falls below its initial value. There is every reason to believe that when $\mathbf{H}$ has risen to saturation value μ' will have fallen to the value unity. It is evident that the presence of direct-current magnetization (above the maximum permeability point) may produce very serious reduction in the effective permeability for small cycles. The effect for silicon iron turns out to be very similar to that shown for ordinary hard and soft iron in the experiments of the late Lord Rayleigh.*

7. *Tests at Telephonic Frequency.*

The mutual inductance method is easily adapted to measurements with superimposed direct-current magnetization, which we may designate the " main field." The main field and the small alternating field can be measured quite independently, as shown in Fig. 5.

The part comprising the iron ring G, the mutual inductometer M, the resistance Q (with potential slider) and the telephone T is the same as before, with the addition of the condenser C in the telephone circuit to prevent direct current from getting through the secondary winding of G round the telephone circuit. The small alternating current I_1 passing through the primary winding of G is measured by means of the thermoammeter A connected through the air core trans-

* Phil. Mag., 1887. Reference ought also to be made to Prof. E. Wilson's experiments on the magnetization of stalloy when screened from the earth's field. (Proc. Roy. Soc., Vol. 90, 1914, and Vol. 93, 1917.)

former a, the combination being calibrated for the frequency used, which was 800 ∿ per second. The source of the alternating current is an electric trumpet buzzer Z linked to the circuit by the variable mutual inductance X. This transformer does not contain iron, in order to ensure that direct current through its secondary coil shall not affect the alternating component. K is an adjustable condenser to give approximate tuning, so that the magnetizing current I_1 shall be nearly of sine wave form.

The direct-current component I_0 is introduced by switching from D to E, thereby bringing the battery b into the circuit. I_0 is measured by the Weston voltmeter Y with low resistance shunt, giving readings that are entirely unaffected by the

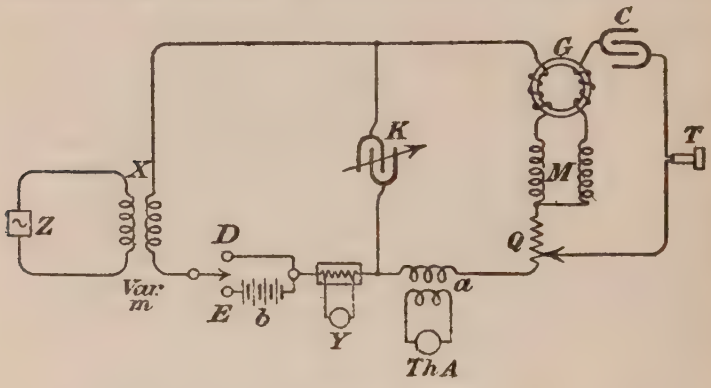


Fig. 5.

presence of the alternating component. In general the alternating component was kept of the order of 3 to 7 milliamperes. For the sheet material this gave cyclic values of $\mathbf{H}$ from ±0·006 up to ±0·011. The applied direct-current field $\mathbf{H}_0$ could be varied from 0·06 up to 2·0. Usually $\mathbf{H}_0$ was reversed eight to ten times after any change, in order to attain a fairly cyclic condition.

The effect of the superimposed field on the permeability μ' was quite marked. As will be seen from the following table (for the sheet material), the changes in μ' at 800 ∿ per second are very similar to those shown in Fig. 3 for very slow small cycles.

Very distinct time effects were observed. With the direct-current field, the value of the effective permeability gradually altered with time, sometimes rising slightly and sometimes

TABLE.

Time conditions of observation.	$\mathbf{H}_0$, direct-current field.	μ' (for small cycles at 800 ω per sec.).
	0·0	260
Immediate	0·18	278
,,	0·50	313
,,	1·00	275
,,	2·09	196
One minute after direct current had been switched off	0·0	247
After standing for 20 hours	0·0	225

falling, according to previous history and the position on the ($\mathbf{H}_0\mathbf{B}$) curve. When the direct-current field was removed, there was also a similar creeping effect, fairly quick at first (*e.g.*, 4 per cent. change in a few minutes), and observable for several hours afterwards, even when the small alternating field was also removed, except for each moment of test. This latter creeping effect appeared always to bring the condition of the material backwards along the ($\mathbf{H}_0\mu'$) curve in Fig. 3. It did not appear to be due to tremors, for the application of fairly vigorous knocking did not seem to have much effect. The slow changes in μ' were easily observed, as the method is sensitive to 2 or 3 parts in 10,000. The effective permeability of the unannealed stalloy wire also showed similar alterations due to direct-current field, and these were in general agreement with the μ' curve of Fig. 4.

In all the experiments the total power loss was measured simultaneously with the permeability μ'. It was found that the power loss was in general altered by the imposition of the direct-current field, but the whole of this change was accounted for by the observed change in permeability.

As already mentioned the hysteresis loss for small values of $\mathbf{H}$ is practically the same in the unannealed as in the annealed material. In this connection it is interesting to mention that Mr. N. H. Williams ("Physical Review," New Series, Vol. 1, May, 1913) has observed that for small cycles of $\mathbf{H}$ (such as are here considered) there is little difference in the hysteresis losses for soft iron and glass hard steel.

In conclusion I would express my thanks to the Admiralty for kind permission to publish the above research, and to Mr. D. W. Dye for kind help in some of the experiments.

ABSTRACT.

Measurements are described of the hysteresis-losses in silicon iron sheet and wires in very low alternating magnetic fields at low and telephonic frequencies using an alternating current method described in a former Paper. The equations giving the hysteresis losses as a function of B_{max} are deduced in the case of the sheet material at low frequencies for ranges of H_{max} from 0·0002 to 0·02.

Comparisons are made between sheet material and wires of different diameters, and curves are given showing the great improvement in the permeability of wires when they are annealed.

The behaviour of the material is studied, both by ballistic tests and at telephonic frequencies, as regards the alternating field when direct current fields of various values are applied at the same time.

DISCUSSION.

Mr. Pollock said he was much interested in the method of measurement and would try it at the first opportunity. He had usually employed modified Bridge methods and had got down to magnetising fields of 0·02, but not so far down as the author. He was interested in the formula for eddy current losses. He had frequently found in measurements with fine wire cores that the eddy current loss takes jumps due to the E.M.F.s set up being sufficient to make the currents pass from one strand to another, and he thought it was impossible to calculate the eddy current losses by any formula. It was important in practice when measuring the losses in stranded cores to take account of the value of the magnetic field to which the core would be subject in use, for with some fields the eddy currents would pass from strand to strand, while with others they would not. He was interested in the conclusion that hysteresis loss was proportional to the square of the flux density. He had had the impression that the loss became very small at very low fields. With regard to Fig. 2, showing the permeabilities for unannealed and annealed stalloy, it was most important in telephone work that the cores of the loading coils should be of constant permeability over the range of field to which they were subject in use. Unfortunately such cores were liable to damage by excessive magnetisation due to accidental currents flowing through the coils, as their permeability did not return to its initial value. Many of the old coils had lost about 15 per cent. of their inductance from such causes. This could only be recovered by re-annealing and unfortunately annealed stalloy had not constant permeability. On this account the Americans were beginning to use granular cores.

Mr. Ritter asked if the windings were absolutely dry, as he had found that a trace of moisture in the dielectric caused quite appreciable losses. He emphasised the importance of constant permeability in the iron used for loading coils. When a cable was loaded the impedance depended on the square root of the ratio of the inductance to the capacity per unit length. If the impedance altered with the current, difficulties were introduced, especially since the use of amplifiers became general.

Mr. Dye said the greatest care was always taken in winding the coils to prevent the possibility of moisture being present. As regards the telephone coils, was the iron in its virgin state to start with? If the cores were magnetised strongly to begin with and then demagnetised, the properties in that state being used as the basis of design, it would always be possible to regain the initial condition without re-annealing.

The Author (communicated): In reply to Mr. Pollock, it still seems probable that by using a suitable system of demagnetisation the bad effects of accidental strong magnetisation can be entirely wiped out. Gumlich and

Rogowski (" Annalen d. Physik," p. 235, Vol. XXXIV. (2), 1911), who worked with low values of **H**, seemed well satisfied with the results they got by demagnetising with a special apparatus giving extremely smooth reduction of the magnetising field. In the experiments described in the Paper there was some suspicion of magnetic ageing at room temperature in the case of the annealed wires. With good stalloy sheet this effect would be improbable, but the material from which the wires are drawn may quite possibly have this bad property. Further investigation seems desirable.

A Demonstration of Experiments on the Thermionic Properties of Hot Filaments. *By* Dr. F. Lloyd Hopwood.

Shown at the Meeting of May 14th, 1920.

The experiments shown were some of those described by Dr. Hopwood in the "Philosophical Magazine," March, 1915, page 362, in which the glowing filament of a carbon lamp and glowing filaments of nichrome and platinum in air are made to move under the influence of positively and negatively charged rods brought into or withdrawn from their vicinity; the character of the effects observed being such as to give a qualitative indication of the thermionic emission from the filaments. In addition, he showed a type of tilted electroscope in which the gold leaf was replaced by a narrow loop of Wollaston wire. When a current is passed through the wire so as to make it glow, it forms an electroscope of different sensitivity for +ve and −ve charges.

DISCUSSION.

Mr. W. R. Cooper asked if the experiments had been tried with lamps lacquered with coloured lacquers. He had seen one covered with a red lacquer, the filament of which would move about following movements of the finger outside.

Dr. D. Owen asked what was the explanation of the asymmetry of the electroscope. Did it depend on the direction of the current in the filament, or did it work as well with alternating current?

Dr. Hopwood said it was possible with lacquered lamps that one got an adventitious charge on the globe which reacted on the filament. He mentioned that during the war he knew of a number of lamps which in order to obscure them were dipped in "tantalum paint." This was so easily electrified when touched by dry hands that about 50 per cent. of the filaments broke.

In reply to Dr. Owen, the electroscope worked equally well with alternating current.

DISCUSSION ON EINSTEIN'S THEORY OF RELATIVITY.

Held March 26, 1920.

Prof. A. S. Eddington said: The subject which we are to discuss arises out of a very famous experiment called the Michelson-Morley experiment, carried out in the year 1887. It was an attempt to detect and measure our motion through the luminiferous aether.

As the earth moves round the sun during the year, so its velocity through the aether must vary. The aether then is streaming past us, at least at some time during the year, and suppose for definiteness the current at the present moment is down this hall. Let a ray of light start from the back of the hall and travel a measured distance to a mirror up here, be reflected, and travel back to the starting point. I think you will easily see that it will take longer—be delayed on account of the current. Suppose at the same time we send another ray of light an equal measured distance across the hall and back. This also is delayed by the current, but it is easy to calculate that the delay is less in this case. We start the two rays together and let them have a race; the across-stream beam is the least delayed, and ought to arrive back first. Actually, when the experiment was tried with all possible refinements, the result was a dead-heat.

To make the conditions perfectly fair, the two courses, which were marked out in the apparatus used, could be interchanged by rotating the apparatus through a right angle. What had been the up-and-down arm became the transverse arm, but still the result was a dead-heat.

There is only one way out of this strange result, and it was suggested by FitzGerald. When we turn an arm of the apparatus, which was originally across stream, so that it is along stream, it must automatically contract by just the amount which compensates for the greater delay of the ray describing this course. And so, whichever arm of the apparatus is placed up stream, it immediately becomes the shorter.

We had been keeping an eye only on the effect of the current on the light waves; but the current may also have an effect on our apparatus. It appears that it has the effect of making a minute change in its dimensions. The change is the same whatever the material of the apparatus, and it is exactly of the amount needed to conceal the effect looked for.

When we consider the matter carefully, it is not so surprising after all. The size and shape of the material apparatus is maintained by the forces of cohesion, which are, presumably, of an electrical nature, and have their seat in the aether. It will not be a matter of indifference how the aether is streaming past, and there will be a readjustment of the molecules when the flow is altered.

But what does seem surprising is that the readjustment of size should just hide the effect we were hoping to find. It looks almost like a conspiracy. There have been three or four other experiments tried since then—of more technical kinds—all in the hope of detecting our motion through the aether; but they have all been defeated by the same kind of conspiracy. And to turn from experiment to theory, we find the same kind of conspiracy even in our mathematical equations. Perhaps you know how sometimes, just as you are hoping to get an important result, the equations evade you, and,

instead of telling you what x is, they announce the solemn but irritating truth that $0=0$. It is somewhat like this in our electromagnetic theory: the velocity through the aether appears abundantly in the formulæ, but, whenever we try to run it down, it drops out, and refuses to be equal to anything in particular

In consequence of these conspiracies, a great generalisation has been put forward, known as the restricted Principle of Relativity. *It is impossible by any conceivable experiment to detect the velocity of a system through the aether.* That is to say, the conspiracy is a general one; and, in fact, it is in the nature of things that this motion is undetectable.

We make this generalisation and build a branch of science on it, in the same way as we make the generalisation that it is impossible to construct a perpetual-motion machine, and build the science of thermodynamics upon it. The case for the Principle of Relativity seems to me as strong as the case for the denial of the possibility of perpetual-motion machines.

We have seen that the concealment of our motion through the aether involves changes of length of moving material. I can best explain the nature of these changes by considering a rather extreme case. The statements I am going to make are all precise mathematical deductions from the principle just enunciated.

Suppose that an aviator flies past us at the rate of 161,000 miles a second. I shall assume that he is in a comfortable travelling conveyance, in which he can move and act normally, and that his length is in the direction of flight. If we could catch a glimpse of him as he passed, we should see a figure about 3 ft. high, but with the width and girth of a normal human being. And the strange thing is that he would be entirely unconscious of his undignified appearance. Looking in a mirror in the car he would see his usual figure. Moreover, if he looked down on us, it would appear to him that we were distorted and had undergone the flattening process.

If we watched the aviator closely, we should infer that he was unusually slow in his movements, and that all events in the car round him were similarly retarded, as though time had half-forgotten to go on. His cigar would last twice as long as one of ours. This slowing down remains after we have applied corrections for the time of transit of the light coming from him. (There is an additional apparent slowing due to the Doppler effect which has been allowed for.)

Here, again, reciprocity comes in, and the aviator perceives nothing unusual in his own surroundings, but thinks that we have gone sluggish. Our cigar lasts twice as long as his.

Which is right? Are we or the aviator? Each of us is using a different reckoning of space and time. To assert that we are right implies that space and time according to our reckoning are *the* space and time, and the aviator's space and time are mathematical fictions; but, owing to the complete reciprocity, this distinction corresponds to nothing experimental. We shall take the view that there is no absolute space and time in Nature, but only space and time reckonings for different observers, which are all on the same footing. The terms space and time only have a meaning relative to an observer, and differ according to his motion.

All this becomes easy to explain if the reality is four-dimensional, and the objects seen by us and the aviator are different three-dimensional appearances or sections of the same four-dimensional things. When a penny is viewed from different positions the observers see different two-dimensional appearances; but it would be absurd to maintain that one of these appearances was the real object, and that the other observers must make corrections on account of their positions. So when we see and measure an oblong block, and the aviator observing the same object finds it to be a cube, it is absurd to suggest that the oblong block is the real object, and that the aviator must correct for his motion. The cube and oblong are only different appearances of the one object, which is four-dimensional. It may

be asked why do we not realise the four-dimensional world as vividly as we realise its three-dimensional section? The reason is that we have two eyes, which are continually telling us that the world has to be looked at from more han one position, and the brain has responded and evolved the perception of solid relief which enables us to synthesise the appearances from different positions. But we are not similarly reminded that it has to be viewed from different motions. Perhaps if we were endowed with pairs of eyes in rapid motion relative to one another we should have evolved a faculty for combining appearances with different motions, and so perceive the relief in the fourth dimension.

In the world of four dimensions, length and duration are components of a single extension between two events. This extension is the same for everybody, being intrinsic in Nature. But observers, according to their motions, take different directions for resolving it into its two components length and duration.

The observer himself is a four-dimensional object: his shape is very elongated—practically linear. As we more usually say, he has considerable extension in time and more moderate dimensions in space. It is natural that the order of events which is parallel with his length should be distinguished by him from the remaining dimensions of the world. He distinguishes that order as time. Each observer takes for time the direction of his own extension—which, from the ordinary point of view, means his track of motion through the world.

If we view Nature in itself abstracting everything which is merely contributed by the observer, not only is space fused with time, but the more familiar attributes of both have vanished. Indeed, it is fair to say that space and time are contributed by the observer, and what is left in Nature is only a certain ordering of events, which is scarcely recognisable as space or time. There is no such thing as shape, no distinction of straight and crooked, no measure of an angle. We cannot describe this world in familiar language, because we have abstracted from it those relative aspects which might make it familiar.

Besides revising our conceptions of space and time, Einstein's theory gives altogether new ideas about force. It does not affect the forces of material contact—tension and thrust; but it affects the mysterious field of force—gravitation—which is a sort of latent influence round massive bodies ready to act if any particle enters the field.

The first thing which falls a victim to the new ideas is Newton's first law of motion—or, at any rate, the usual conception of its meaning. Every body moves uniformly in a straight line if undisturbed by forces. I suppose I am right in assuming that we think of this as implying t at a body has an innate tendency called inertia, which makes it move uniformly in a straight line; but this can be overcome by active causes called forces. But I have said there is no distinction between straight and crooked in Nature; it is the observer who decides whether a certain locus shall be considered straight, and there is no such distinction of active and passive tendencies. All we can say is that there are certain natural tracks laid down in the space-time world—tracks which particles actually do follow unless some other material particles hit them. We call these natural tracks *geodesics*; they are laid down in Nature independently of an observer. Their courses must be determined by conditions in the universal substratum of things—the aether, the world, or whatever name you like to give it. For the present it seems useless to analyse them farther; we take these natural tracks and study their laws; this study is the geometry of the world.

What happens if the observer himself deviates from his natural track? He can only do this if something is hitting him. He then perceives that he is in a field of force. The field is simply his own disturbance of path transferred to the objects round him, just as the diurnal revolution of the stars is our own rotation transferred to the stars. The observer gives different

names to the field of force according to the conditions which produce his disturbance. If it is due to the earth whirling him round once a day, he says he is in a field of the earth's centrifugal force. If it is due to the molecules of the ground bombarding the soles of his boots, accelerating him upwards, he says he is in the field of the earth's gravitational force; but these fields are of essentially the same nature. This hypothesis has immediate consequences of great importance. We understand centrifugal force completely, and a little reflection will show that a ray of light is deflected by a field of centrifugal force, just as a material particle with the same velocity would be. (We recognise, of course, that this is a spurious effect due to our own rotation.) It follows at once that light can be deflected by gravitation, since the nature of the two fields is the same.

I suppose that my statement that the gravitation which we see manifested around us is simply due to the ground shoving against our feet requires a little more explanation. We generally agree that there is no absolute motion, and that all one can say is that two things are moving relatively to one another. So, when Newton saw the apple fall, all one can say is that there was a relative acceleration of the apple and Newton, without apportioning the responsibility to either. Still, there may be an easy and a paradoxical way of describing things; and since the apple was entirely free, suspended in space, whereas Newton was being bombarded upwards by he molecules of his chair, it seems the natural thing to attribute the acceleration to Newton. We are taking the geodesics as standard, and attribute accelerations relative to them to active causes—material impacts, about which there is nothing mysterious. To take the straight line as standard has two objections: (1) that it is a locus depending on the observer, without significance in Nature; (2) that the accelerations relative to it must be attributed to an invisible influence called the force of gravitation.

It is difficult to show without mathematics how Einstein proceeded from the consideration of these geodesics to deduce his new law of gravitation. The field of force round the sun can be specified completely if we state the courses in space and time of all possible particles projected in any manner in it. That is what Kepler did. We must find some way of unifying this cumbrous mass of knowledge. We cannot accept Newton's method of accomplishing this, because it introduces things relative to particular observers which we cannot believe have any *locus standi* in the true laws of the external world. Now, all the possible tracks together form a tangled web. A similar sort of web is formed by all the lines of shortest (or longest) length in a non-Euclidean space. If we can correlate such a non-Euclidean space, so that one web is an exact reproduction of the other, then we can obtain a concise summary of the whole mass of detail by merely stating the kind of geometry of the correlated space. Since there is some law governing the tracks, there must be some limitation on the correlated geometry. When we consider the possible ways of classifying non-Euclidean geometries, we find there are not many broad divisions; and there is only one reasonably likely law of gravitation, or limitation on the geometry, suggested. By proceeding in this way we confine attention to those things which have an intrinsic significance in Nature, and keep out things which appear only in knowledge relative to some observer, which are obviously unsuitable in formulating a general law of Nature.

The law thus suggested turns out to be almost equivalent to Newton's law in all ordinary applications; and in the exceptional cases where they differ appreciably—the deflection of light and the motion of perihelion of Mercury—it is the new law which is confirmed.

I have spoken of the non-Euclidean space as *correlated* with the field of gravitation. It turns out, however, that this space must actually be identical with the space and time of perception and of physical measurement—at any rate, far beyond possible experimental accuracy. It is in connection with

this point that the third crucial test—the displacement of the Fraunhofer lines—comes in.

Probably this final identification of the field of force round the sun with the non-Euclidean properties of the physical space-time which we perceive there is the most difficult part of this theory. The essential point is that there is something going on in Nature whose absolute character we cannot attempt to define; we can only describe it by the effects to which it leads in our physical measures. Now, this absolute thing has effects of different kinds. Fields of force are one of these effects, and if we describe in terms of fields of force, we get a mechanical description of Nature, not very unlike Newton's description of gravitational force. The pulsations of clocks and the extensions of material scales measure other effects of the same thing, and if we describe in terms of these we get a geometrical description of Nature with non-Euclidean space and time. It is the strength of Einstein's theory that it brings out the unity underlying the two descriptions; that, since it is the same thing in Nature that is at the back of both, we can use the same description both for those aspects of Nature which are usually regarded as mechanical and those which are regarded as geometrical; and it is through this unification that the theory has achieved such great progress.

Mr. Bolton: Prof. Eddington mentioned that to an aviator flying at a high speed people on the ground would appear contracted in the direction of motion and *vice versa*, and appeared to deduce from this that certain dimensions of the interferometer in the Michelson-Morley Experiment would be contracted. He did not see the connection, as there was no relative motion between the observer and the instrument.

Prof. Eddington pointed out that the experiment gave a null result because of this fact; but the mathematician had deduced the contraction by imagining himself at rest relative to the æther, and therefore having a velocity relative to the instrument.

Sir George Lane Fox-Pitt asked if the observer was always regarded merely as a seeing person rather than a thinking person.

Prof. Eddington said that by "observer" he meant a person armed with his perceptions and with scientific apparatus where necessary to aid him in making measurements.

Dr. H. Levy said it was difficult to see why of all the possible tracks which a particle might follow it should always elect to follow a geodesic.

Prof. Eddington said one reason for identifying the natural tracks of particles with geodesics was that a particle must take a unique track definable without reference to an observer. The only track which can be defined uniquely in this way is a geodesic.

Mr. T. Smith asked if light in a gravitational field became to any extent polarised.

Prof. Eddington did not think there was any evidence of this.

Prof. A. O. Rankine said, apropos of Mr. Smith's question, that a few months ago in collaboration with Dr. Silberstein he had carried out some experiments on this point, the results of which were in the course of publication. Silberstein had shown that according to Einstein's theory a gravitational field should produce an effect on the velocity of light polarised in a plane parallel to the field. The same theory showed, however, that light plane-polarised at right angles to the field would be affected to the same extent, and it was desired to test how far this equivalence was in accordance with fact. A beam of light, plane polarised at 45° to the vertical, was projected in a horizontal direction and examined for ellipticity some distance away. It is evident that if the vertical and horizontal components travelled at different rates, a phase difference would be introduced which would result in elliptical polarisation. No trace of ellipticity was detected, the deduction from Einstein's theory being confirmed. Perhaps the most

interesting fact emerging from the measurements was the extraordinary degree of precision with which the two velocities were found to be equal. The difference of optical path of the two components was certainly less than one three hundredth of a millimetre in the distance travelled in a second., viz., three hundred thousand kilometres, assuming a gravitational field equal to that of the earth to hold over the whole distance.

Dr. VINCENT asked if the theory of Relativity applied to the Kinetic Theory of Gases would not oblige us to alter our ideas on high temperatures. The conclusion was probably fallacious, but it appeared at first sight as if the electron could not move with a velocity greater than that of light, and that consequently there would be an upper as well as a lower limit to the absolute temperature scale.

Prof. EDDINGTON replied that with our usual definition of temperature as proportional to the energy of the atoms there was no upper limit; but if the scale were chosen to depend not on the energy but on the square of the velocity, it would reach a finite limit.

SIR JOSEPH LARMOR, in a communication read by the Secretary, said the charm of the Einstein train of ideas seems to be traceable to several causes. In the first place it has revived the interest in the Riemann differential geometry, itself a transcendental development of the ideas of the Gaussian theory of surfaces, and the admiration of Gauss for the train of thought of his disciple is on record. Then again, as it loosens all foundations, its ideas are capable of development in unexpected directions. How far it is from being a determinate theory of the universe will appear to anyone who dips into the writings of its developers. Indeed, if it became fixed and rigid and ceased to reveal unexpected vistas, much of the fascination would be gone. For example, the crowning consideration for the most general relativity, on which special stress is laid by Lorentz, and by Eddington, is the assertion of Einstein that it is only the intersections of the world-lines of particles that can constitute events, and so long as these points retain the same order along their lines, we may give body to their collocation by filling up with any kind of intermediate spacial scaffolding that we please. There is nothing to restrict it at all. Yet for all that we proceed straightway to assume a space with its ten gravitational potential functions, and work out its mathematics in the necessary elaboration; while we learn that ultimately there can really be nothing to limit our choice of such a framework. I observe that Langevin, being a Frenchman, does not shrink from blunt formulation of the full logical consequence. It is, therefore, possible, says he (Société française de Physique, Feb. 6th, 1920), to make enunciation of the principle of generalised relativity, in the assertion that the laws of physics have the same form for all systems of reference, in motion (or deformation) anyhow, in virtue of the introduction in each case of its own special field of gravitation suitably distributed. The ensuing problem is then to explain how it is that astronomers, since Newton's time, have persisted in one special and very precise illusion about the distribution of gravitation, whereas really an unlimited choice was open. One answer seems to involve swinging over to the opposite point of view, to the youthful dogmatism of Clifford, that if space could be fully known the whole material universe would be found to be involved implicitly within it. The general dialectic can thus develop many-sided and indefinite; the necessary coordinating consideration appears to be present in a somewhat latent postulate of one space-time continuum, which this unlimited multiplicity constitutes merely different modes of describing. If we would thus reach at bottom an underlying scheme of space and time after the manner of Newton, why should the natural historical order of development in terms of an æther be ruled out *a priori*?

The guiding philosopher for this type of theory is claimed by Einstein to be Hume, filtered through Mach; and it certainly affords a wider field for the play of his scepticism than his own illustrations.

The primary consideration in all sceptical analysis is to know where to stop, and, as with Kant in Hume's time, to replace further destructive criticism by constructive hypothesis. For the unresolvable essence of relativity appears to be that we cannot get on without assuming *some foundation* to which phenomena are referred, and with respect to which they are ordered to the degree that is necessary for our reasonings.

It has not escaped the notice of the creators of the analytical theory that the whole of it is expressible as a corollary, of novel type it is true, to the fundamental physical principle of least action, which adopts that principle to take in the phenomena of gravitation so as to place them in analytical relation to the other physical principles of nature. Of the fundamental character of that principle Helmholte, following upon Hamilton and Kelvin and Maxwell, was not the least of the exponents. The original criteria of stress-energy-momentum tensors then shrink to their proper subordinate position. There is then nothing revolutionary about this brilliant theory, unless it be the fiction that time is merely an additional dimension of space, which is inconsistent with the very language in which it has to express itself, and at variance with the different conditions for physical determination in space and in time. That it is convenient to use mixed co-ordinates of time and space is another matter whose bearings were beautifully elucidated by the application long ago of the expressive phrase *local time* by Lozentz: it is local time varying irregularly from place to place that allows gravitation to come within the least action scheme.

Mr. A. W. Ravenshear said that in the Michelson-Morley experiment the contraction of the apparatus could not be measured by measuring rods or anything of that kind. It therefore required reference to some other system of measurement. What was this?

Prof. Eddington repeated that no contraction could be directly measured by this experiment. We are led to expect it on the assumption that the apparatus is moving relative to the æther. At any moment we do not know whether we are moving relative to the æther or not; but at some parts of the year we must be, so that if the results of the experiment are always negative we have to deduce the contraction.

A Speaker asked how, if curvatures in space only exists round matter, accommodation was found in the theory for the existence of free energy.

Prof. Eddington remarked that energy also gave curvature.

Mr. C. O. Bartrum pointed out that the Michelson-Morley experiment compared the velocity of light athwart the æther stream with the mean of the velocities up and down the stream. Had any experiment been made comparing the velocity up the stream with that down the stream, and did Prof. Eddington consider such an experiment was possible? He asked the question because he had seen it stated that the work of M. Q. Majorana had shown these velocities to be equal.

Prof. Eddington said he had heard that statement, but was not in agreement with it. Majorana's work was to show that there was no change in the velocity relative to the observer due to motion of the source or of a reflecting mirror. He did not think an experiment could be devised to test the point referred to as it was necessary to determine the times of arrival and departure at the two ends of the path. This could only be done by clocks, and if you use only one, you must correct for the velocity of light, which is the thing you are trying to measure.

Mr. T. Smith asked if any of the conclusions of the theory would be modified if we were assumed to be the dealing with group velocities rather than wave velocities. If he remembered rightly the most accurate determinations had revealed a difference even in vacuum.

Prof. Eddington was unable to say offhand.

Dr. E. H. Rayner asked what happened if the velocity of a body exceeded the velocity of light.

Prof. Eddington pointed out that a body would require infinite energy to attain this velocity, and so could not exceed it.

XXIV. *On Tracing Rays through an Optical System. (Third Paper).* *By* T. SMITH, *B.A.* (*From the National Physical Laboratory.*)

THE great majority of the rays that are traced by optical computers through a train of lenses lie in a plane containing the axis. The calculations for such rays are very simple compared with those for rays not remaining in the same plane during their passage through the system, but are distinctly more laborious than for those rays which lie close to the optical axis. The equation connecting conjugate axial points when refraction takes place at a single surface may be modified* in a variety of ways to meet the case of rays reaching the surface at large angles and at considerable distances from the axis. It has been pointed out that in some special cases these modified equations may provide a more rapid means of tracing rays than the usual trigonometrical methods. The object of the present note is to illustrate how such equations may be fitted into a scheme for ray tracing of quite general applicability.

The usual equation for the refraction of paraxial rays is

$$\frac{\mu'}{x'}-\frac{\mu}{x}=\frac{\mu'-\mu}{r}=(\mu'-\mu)R=\kappa,$$

or if m is the transverse magnification

$$x\kappa=-\mu(1-1/m), \quad x'\kappa=\mu'(1-m).$$

One of the generalised forms is

$$\frac{\mu'}{x'-p}-\frac{\mu}{x-p}=\frac{\mu'-\mu}{r-p}=j, \qquad \ldots\ldots \quad (1)$$

or if $\mathfrak{M}$ is the sine ratio $\mu \sin\psi/\mu'\sin\psi'$, where ψ and ψ' are the inclinations of the ray to the axis before and after refraction,

$$(x-p)j=-\mu(1-1/\mathfrak{M}), \quad (x'-p)j=\mu'(1-\mathfrak{M}) \quad \ldots \quad (2)$$

Such forms may be justified immediately, for the sine ratio depends only on the relative distances of the object and image intersection points from the centre of curvature, and is, therefore, undisturbed by replacing the actual refraction by paraxial refraction at a concentric surface; moreover, by changing the radius of curvature the two conjugate points may be made

* Trans. Opt. Soc., Vol. 20., p. 23.

conjugate for paraxial refraction. The satisfaction of these conditions evidently defines the refraction completely.

It has been shown* previously that whenever refraction in an axial plane takes place without aberration, the perpendiculars to the ray from the vertex of the refracting surface before and after refraction are equal. If, then, refraction according to the paraxial law is to account satisfactorily for this non-paraxial ray, the vertex must be so chosen that these two perpendiculars are equal. This result also follows at once from equations (2) if they are multiplied by sin ψ and sin ψ' respectively. If, as before, the perpendiculars from the actual vertex are denoted by $\mathfrak{h}$ and $\mathfrak{h}'$,

$$\mathfrak{h}+p\sin\psi=\mathfrak{h}'+p\sin\psi', \quad . \quad . \quad . \quad . \quad (3)$$

and since the refraction follows paraxial laws exactly

$$\mu'\sin\psi'-\mu\sin\psi=-j(\mathfrak{h}+p\sin\psi). \quad . \quad . \quad . \quad (4)$$

Equations (3) and (4) are equivalent to (2), but are in a more desirable form for ray tracing since $\mathfrak{h}$ and $\mathfrak{h}'$ are always finite, while x and x' may attain any real values.

If the angular aperture at which refraction takes place is -2α. the deviation is 2δ, and the angle made with the axis by the join of the intersection of the rays with the aplanatic surfaces is 2γ, the inclinations ψ, ψ' to the axis and the angles φ, φ' with the normal are given by

$$\psi=\gamma-\alpha-\delta,\ \psi'=\gamma-\alpha+\delta,\ \varphi=\gamma+\alpha-\delta,\ \varphi'=\gamma+\alpha+\delta. \quad . \quad (5)$$

Since the equivalent vertex is the point where the external bisector of the angle made by the rays meets the axis, it is easily seen that

$$\frac{r-p}{r}=\frac{\cos(\gamma+\alpha)}{\cos(\gamma-\alpha)},$$

or

$$p=\frac{2er}{1+e^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

where

$$e=\tan\alpha\tan\gamma. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

This is the equation given previously for finding p. Evidently p will ordinarily be a very small quantity. In certain cases its value may be written down with sufficient accuracy without calculation, but in general it must be determined from the known values of $\mathfrak{h}$ and sin ψ. The equation expressing it in terms of these variables is a bi-quadratic. This would be expected, since there will be solutions corresponding to both

* Proc. Phys. Soc., Vol. XXX., p. 227.

the internal and the external bisectors of the angle between the rays, and two further solutions corresponding to refraction at a surface of opposite curvature, which may evidently satisfy the conditions. The high order of this equation occasions no difficulty, as the small root required is given very approximately by the two lowest terms of the equation, the remaining terms of which may be regarded as corrections to this approximate value. The value of p so found may be applied to (4) to find $\sin\psi'$, and the value of $\mathfrak{h}$ for the next surface may be determined by applying the usual equation of transference, reckoning the separation from the equivalent vertex.

The object of this method of calculation is to avoid the use of the equation $\psi'=\psi-\varphi+\varphi'$ applied to the angles themselves, this, the essential characteristic of the trigonometrical method, necessarily involving four references to tables, and usually an interpolation for each. The value of any alternative procedure must lie in its furnishing the value of $\sin\psi'$ to a corresponding degree of accuracy in less time.

From the known values of $\sin\psi$, $\mathfrak{h}$ and R, the sines of the angles of incidence and emergence are found from the usual formula

$$\mu'\sin\varphi'=\mu\sin\varphi=\mu(\mathfrak{h}R+\sin\psi). \quad . \quad . \quad . \quad (8)$$

It is then evident from equations (5) that to a first approximation

$$2\alpha=\sin\varphi-\sin\psi, \quad 2\gamma=\sin\varphi'+\sin\psi,$$

and therefore

$$4e=\mathfrak{h}R(\sin\varphi'+\sin\psi),\ 2p=\mathfrak{h}(\sin\varphi'+\sin\psi). \quad . \quad (9)$$

This is an underestimate for p, the exact relation being

$$2p=\frac{\mathfrak{h}(\sin\varphi'+\sin\psi)}{\cos(\gamma-\delta)\cos(\gamma-\alpha)\cos(\alpha+\delta)},$$

which suggests as a corrected form

$$2p=\frac{\mathfrak{h}(\sin\varphi'+\sin\psi)}{1-\frac{1}{8}\{(\sin\varphi+\sin\psi)^2+(\sin\varphi'-\sin\psi)^2+(\sin\varphi'-\sin\varphi+2\sin\psi)^2\}} \quad (10)$$

and this is sufficiently accurate for all ordinary purposes. Other forms may be found without difficulty by constructing equations satisfied by e. Thus from equations (5)

$$\sin\varphi-\sin\psi=2\sin\alpha\cos(\gamma-\delta),\ \sin\varphi+\sin\psi=2\cos\alpha\sin(\gamma-\delta)$$
$$\sin\varphi'+\sin\psi=2\sin\gamma\cos(\alpha+\delta),\ \sin\varphi'-\sin\psi=2\cos\gamma\sin(\alpha+\delta).$$

Combining these equations with e

$$\sin\varphi-\sin\psi+e(\sin\varphi+\sin\psi)=2\sin\alpha\cos\delta/\cos\gamma,$$
$$\sin\varphi'+\sin\psi+e(\sin\varphi'-\sin\psi)=2\sin\gamma\cos\delta/\cos\alpha,$$
$$\sin\varphi-\sin\psi-e(\sin\varphi'-\sin\psi)=2\tan\alpha\cos(\alpha+\gamma)\cos\delta,$$
$$\sin\varphi'+\sin\psi-e(\sin\varphi+\sin\psi)=2\tan\gamma\cos(\alpha+\gamma)\cos\delta,$$
$$(\sin\varphi-\sin\psi)(\sin\varphi'+\sin\psi)-e(\sin\varphi+\sin\psi)(\sin\varphi'-\sin\psi)=4\sin\alpha\sin\gamma\cos(\alpha+\gamma),$$
$$(\sin\varphi-\sin\psi)(\sin\varphi'+\sin\psi)-e^2(\sin\varphi+\sin\psi)(\sin\varphi'-\sin\psi)=4e\cos(\alpha+\gamma)\cos\delta\cos\psi,$$

from which $\cos(\alpha+\gamma)$ and $\cos\delta$ have to be eliminated. Thus

$$4e[\sin\varphi-\sin\psi-e(\sin\varphi'-\sin\psi)][\sin\varphi'+\sin\psi-e(\sin\varphi+\sin\psi)]$$
$$=(1-e)[\sin\varphi-\sin\psi+e(\sin\varphi+\sin\psi)][\sin\varphi'+\sin\psi+e(\sin\varphi'-\sin\psi)]$$
$$\times[(\sin\varphi-\sin\psi)(\sin\varphi'+\sin\psi)-e(\sin\varphi+\sin\psi)(\sin\varphi'-\sin\psi)]$$
$$=[(\sin\varphi-\sin\psi)(\sin\varphi'+\sin\psi)-e^2(\sin\varphi+\sin\psi)(\sin\varphi'-\sin\psi)]^2\sec^2\psi.$$

The second of these equations is ordinarily to be preferred. Let the correcting factor $\mathcal{E}$ be taken as independent variable, where

$$4e\mathcal{E}=(\sin\varphi-\sin\psi)(\sin\varphi'+\sin\psi) \quad . \quad . \quad . \quad . \quad (11)$$

The second equation then becomes

$$\mathcal{E}[\mathcal{E}^2-\tfrac{1}{4}\mathcal{E}(\sin^2\varphi+\sin^2\varphi'-2\sin^2\psi)+\tfrac{1}{16}(\sin^2\varphi-\sin^2\psi)(\sin^2\varphi'-\sin^2\psi)]\cos^2\psi$$
$$=[\mathcal{E}^2-\tfrac{1}{16}(\sin^2\varphi-\sin^2\psi)(\sin^2\varphi'-\sin^2\psi)]^2 \quad . \quad . \quad . \quad (12)$$

with the solution

$$\mathcal{E}=1-\tfrac{1}{4}(\sin^2\varphi+\sin^2\varphi'+2\sin^2\psi)-\tfrac{1}{16}(\sin^4\varphi+\sin^4\varphi'+\sin^4\psi-\sin^2\varphi\sin^2\varphi'-\sin^2\varphi\sin^2\psi-\sin^2\varphi'\sin^2\psi)-\ldots.$$

The terms of the fourth order are obviously small in many cases, and since $\mathcal{E}$ is a correcting factor to a small quantity, they may often be neglected. The value of p is to be obtained from

$$2p=\frac{h(\sin\varphi'+\sin\psi)}{\mathcal{E}+\frac{1}{4}hR(\sin\varphi'+\sin\psi)}, \quad . \quad . \quad . \quad (13)$$

which agrees with (10) when terms of the fourth order in $\mathcal{E}$ are neglected.

One of the advantages to be expected from tracing rays by calculating the p's of successive surfaces is that under similar conditions of refraction the values of p are closely proportional to the thicknesses of the lenses if these are determined by the lens aperture, and are independent of the focal length. It will

thus often happen that a computer who employs this system will be able from his past experience to assume sufficiently accurate values for p by basing them on the lens thicknesses. Nevertheless it is at times convenient to disregard p entirely, and to eliminate it from equations (3) and (4). It will be shown that this leads to a correcting factor quite symmetrical in φ, φ' and ψ.

Since there is no aberration when $\hbar = 0$, it is permissible to assume for p the form

$$p = \frac{\hbar P}{1 - P \sin \psi}, \quad \ldots \ldots \quad (14)$$

which gives

$$\hbar + p \sin \psi = \frac{\hbar}{1 - P \sin \psi}$$

and

$$\frac{r - p}{r} = \frac{1 - P \sin \varphi}{1 - P \sin \psi}$$

giving for (4)

$$\mu' \sin \psi' - \mu \sin \psi = -\kappa \hbar / (1 - P \sin \varphi). \quad \ldots \quad (15)$$

In this form the equation is not inconvenient for use, but an alternative form free from refractive indices

$$\sin \psi' - \sin \psi = (\sin \varphi' - \sin \varphi) \frac{1 - P \sin \psi}{1 - P \sin \varphi} \quad \ldots \quad (16)$$

is obtained by adding $(\mu - \mu') \sin \psi$ to both sides. By substituting in (3) this value of $\sin \psi'$ and by the original assumption the two equations

$$\frac{\hbar'}{\hbar} = \frac{1 - P \sin \psi'}{1 - P \sin \psi} = \frac{1 - P \sin \varphi'}{1 - P \sin \varphi} \quad \ldots \quad (17)$$

for $\hbar'$ are found. It remains to find a formula for P. Since P vanishes with $\sin \varphi' + \sin \psi$, put

$$2PE = \sin \varphi' + \sin \psi \quad \ldots \ldots \quad (18)$$

Comparing (13) and (14)

$$E = \mathfrak{E} + \tfrac{1}{4}(\sin \varphi + \sin \psi)(\sin \varphi' + \sin \psi) \quad \ldots \quad (19)$$

and the elimination of $\mathfrak{E}$ between (12) and (19) gives

$$E^4 - E^3(1 + s_2) + \tfrac{1}{4}E^2(s_2^2 + s_1 s_3 + s_1^2 + s_2) - \tfrac{1}{8}E(s_1 + s_3)(s_1 s_2 - s_3) + \tfrac{1}{64}(s_1 s_2 - s_3)^2 = 0 \quad (20)$$

where

$$s_1 = \sin\varphi + \sin\varphi' + \sin\psi, \quad s_2 = \sin\varphi \sin\varphi' + \sin\varphi \sin\psi + \sin\varphi' \sin\psi,$$
$$s_3 = \sin\varphi \sin\varphi' \sin\psi,$$

showing that E is symmetrical in φ, φ', ψ. This is also seen by noting that

$$E=\cos\alpha\cos\delta\cos(\alpha+\delta)$$
$$=\tfrac{1}{4}\{1+\cos(\varphi-\psi)+\cos(\varphi'-\psi)+\cos(\varphi'-\varphi)\} \quad . \quad (21)$$

From either (20) or (21) it is easy to derive the solution

$$\left.\begin{aligned} E&=1+\tfrac{1}{8}(S_1{}^2-3S_2)+\tfrac{1}{32}(S_2{}^2-3S_4)+\tfrac{1}{64}(S_2S_4-3S_6)+\ldots \\ &=\tfrac{1}{8}[-1+S_1{}^2+(3-\tfrac{1}{2}S_2-\tfrac{1}{8}S_4-\tfrac{1}{16}S_6\ldots)^2]\end{aligned}\right\} (22)$$

where

$$S_n=\sin^n\varphi+\sin^n\varphi'+\sin^n\psi,$$

an exact solution when required being formed from the sum of the three cosines. In numerical calculations equations (16) and (17) will be used in the forms

$$\sin\psi'-\sin\psi=(\sin\varphi'-\sin\varphi)\left[1+\frac{(\sin\varphi-\sin\psi)(\sin\varphi'+\sin\psi)}{2E-\sin\varphi(\sin\varphi'+\sin\psi)}\right]$$

$$\eta'=\eta\left[1+\frac{(\sin\varphi-\sin\varphi')(\sin\varphi'+\sin\psi)}{2E-\sin\varphi(\sin\varphi'+\sin\psi)}\right],$$

the denominator being frequently represented with sufficient accuracy by

$$2-\tfrac{1}{4}[(\sin\varphi+\sin\varphi')^2+(\sin\varphi+\sin\psi)^2+(\sin\varphi'-\sin\psi)^2]$$

where the sines in each term are connected by the opposite sign to the one in the terms of the numerators. It should be noted that the correcting term in the first of these equations is obtained by multiplying that for the second equation by $-R$, as is evident from the initial equation for η' in terms of φ' and ψ'.

When the cosines of the angles are to be found, the denominator will be most simply calculated from the formula

$$\tfrac{1}{4}\{(\sin\psi-\sin\varphi+\sin\varphi')^2+(\cos\psi+\cos\varphi+\cos\varphi')^2-1\}$$

and it is in this form that the equations have been found most convenient in practice. It will be noted that since the difference between any cosine and unity enters into the equations as a correction to the aberration alone and does not affect the normal refraction according to Gaussian laws, a knowledge of the cosine to a comparatively small number of figures yields an accuracy corresponding to the use of a greater number of figures in the usual trigonometrical calculation. It thus becomes possible to obtain a very high degree of accuracy

by constructing a short four-figure table giving the cosines of angles in terms of their sines, such as that given below. The interpolations involved in the use of this table only affect the last figure and can always be made mentally. They thus differ fundamentally from those required with the tables ordinarily used, and the time usually spent in turning over the pages of tables to reach the angles required is entirely avoided. Calculations made by this method on photographic lenses of complex form with angles of incidence reaching 60° show that the final results are accurate to about 3″ in direction, with a corresponding degree of accuracy in the position of the ray. Unlike the methods described previously the perpendicular after refraction is obtained before the new direction of the ray is found. The order of operations is indicated by the equations

$$\sin\varphi_\lambda = \sin\psi_{\lambda-1} + h_\lambda R_\lambda$$

$$\mu_\lambda \sin\varphi'_\lambda = \mu_{\lambda-1}\sin\varphi_\lambda$$

$$h'_\lambda = h_\lambda + \frac{4h_\lambda(\sin\varphi_\lambda - \sin\varphi'_\lambda)(\sin\varphi'_\lambda + \sin\psi_{\lambda-1})}{(\sin\psi_{\lambda-1} - \sin\varphi_\lambda + \sin\varphi'_\lambda)^2 + (\cos\psi_{\lambda-1} + \cos\varphi_\lambda + \cos\varphi'_\lambda)^2 - 1} \qquad (23)$$

$$\sin\psi_\lambda = \sin\varphi'_\lambda - h'_\lambda R_\lambda$$

$$h_{\lambda+1} = h'_\lambda + t_\lambda \sin\psi_\lambda.$$

The formulæ as they stand are applicable to every case that can arise, and it does not appear desirable to depart from them in any case with the possible exception of a flat surface, where the value of $\sin\psi_\lambda$ is obtained from that of $\sin\psi_{\lambda-1}$ by the law of refraction, and h'_λ may be obtained from the relation

$$\frac{h'_\lambda}{\cos\psi_\lambda} = \frac{h_\lambda}{\cos\psi_{\lambda-1}}.$$

If this equation is used the cosines must be calculated directly, the table being insufficiently accurate. If the table is to be utilised the equation should be expressed in the form

$$h'_\lambda = h_\lambda\left[1 + \frac{(\sin\psi_{\lambda-1} - \sin\psi_\lambda)(\sin\psi_{\lambda-1} + \sin\psi_\lambda)}{\cos\psi_{\lambda-1}(\cos\psi_{\lambda-1} + \cos\psi_\lambda)}\right].$$

Perhaps the simplest method of deriving the new equation for h' from those given previously is to start from the relations

$$\frac{h'}{h} = \frac{\sin\varphi' - \sin\psi'}{\sin\varphi - \sin\psi} = \frac{\cos\varphi' + \cos\psi'}{\cos\varphi + \cos\psi},$$

COSINES IN TERMS OF SINES.

	0	1	2	3	4	5	6	7	8	9	D	D^{-1}
·70	·7141	·7132	·7122	·7112	·7102	·7092	·7082	·7072	·7062	·7052	−10	−10
1	·7042	·7032	·7022	·7012	·7001	·6991	·6981	·6971	·6960	·6950	−10	−10
2	·6940	·6929	·6919	·6908	·6898	·6887	·6877	·6866	·6856	·6845	−11	− 9
3	·6834	·6824	·6813	·6802	·6791	·6781	·6770	·6759	·6748	·6737	−11	− 9
4	·6726	·6715	·6704	·6693	·6682	·6671	·6659	·6648	·6637	·6626	−11	− 9
5	·6614	·6603	·6592	·6580	·6569	·6557	·6546	·6534	·6523	·6511	−11	− 9
6	·6499	·6488	·6476	·6464	·6452	·6440	·6428	·6416	·6404	·6392	−12	− 9
7	·6380	·6368	·6356	·6344	·6332	·6320	·6307	·6295	·6283	·6270	−12	− 8
8	·6258	·6245	·6233	·6220	·6208	·6195	·6182	·6170	·6157	·6144	−13	− 8
9	·6131	·6118	·6105	·6092	·6079	·6066	·6053	·6040	·6027	·6013	−13	− 8
·80	·6000	·5987	·5973	·5960	·5946	·5933	·5919	·5906	·5892	·5878	−14	− 7
1	·5864	·5851	·5837	·5823	·5809	·5795	·5781	·5766	·5752	·5738	−14	− 7
2	·5724	·5709	·5695	·5680	·5666	·5651	·5637	·5622	·5607	·5592	−15	− 7
3	·5578	·5563	·5548	·5533	·5518	·5502	·5487	·5472	·5457	·5441	−15	− 7
4	·5426	·5410	·5395	·5379	·5363	·5348	·5332	·5316	·5300	·5284	−16	− 6
5	·5268	·5252	·5235	·5219	·5203	·5186	·5170	·5153	·5136	·5120	−16	− 6
6	·5103	·5086	·5069	·5052	·5035	·5018	·5000	·4983	·4966	·4948	−17	− 6
7	·4931	·4913	·4895	·4877	·4859	·4841	·4823	·4805	·4787	·4768	−18	− 6
8	·4750	·4731	·4712	·4694	·4675	·4656	·4637	·4618	·4598	·4579	−19	− 5
9	·4560	·4540	·4520	·4501	·4481	·4461	·4441	·4420	·4400	·4379	−20	− 5
·90	·4359	·4338	·4317	·4296	·4275	·4254	·4233	·4211	·4190	·4168	−21	− 5
1	·4146	·4124	·4102	·4080	·4057	·4035	·4012	·3989	·3966	·3943	−23	− 4
2	·3919	·3896	·3872	·3848	·3824	·3800	·3775	·3751	·3726	·3701	−24	− 4
3	·3676	·3650	·3625	·3599	·3573	·3546	·3520	·3493	·3466	·3439	−26	− 4
4	·3412	·3384	·3356	·3328	·3299	·3271	·3242	·3212	·3183	·3153	−29	− 3
5	·3122	·3092	·3061	·3030	·2998	·2966	·2934	·2901	·2868	·2834	−32	− 3
6	·2800	·2765	·2730	·2695	·2659	·2622	·2585	·2548	·2510	·2471	−37	− 3
7	·2431	·2391	·2350	·2308	·2265	·2222	·2178	·2132	·2086	·2039	−44	− 2
8	·1990	·1940	·1889	·1836	·1782	·1726	·1667	·1607	·1545	·1479	−53	− 2
9	·1411	·1339	·1262	·1181	·1094	·0999	·0894	·0774	·0632	·0447	...	...
·990	·1411	·1404	·1397	·1389	·1382	·1375	·1368	·1361	·1353	·1346	...	...
1	·1339	·1331	·1324	·1316	·1309	·1301	·1293	·1286	·1278	·1270	...	...
2	·1262	·1254	·1247	·1239	·1231	·1222	·1214	·1206	·1198	·1190	...	...
3	·1181	·1173	·1164	·1156	·1147	·1138	·1130	·1121	·1112	·1103	...	...
4	·1094	·1085	·1075	·1066	·1057	·1047	·1038	·1028	·1018	·1009	...	...
5	·0999	·0989	·0979	·0968	·0958	·0948	·0937	·0926	·0916	·0905	...	...
6	·0894	·0882	·0871	·0859	·0848	·0836	·0824	·0812	·0799	·0787	...	...
7	·0774	·0761	·0748	·0734	·0721	·0707	·0692	·0678	·0663	·0648	...	...
8	·0632	·0616	·0600	·0583	·0565	·0548	·0529	·0510	·0490	·0469	...	...
9	·0447	·0424	·0400	·0374	·0346	·0316	·0283	·0245	·0200	·0141	...	...

[*To face p. 258.*

whence

$$\frac{h'}{h}=\frac{(\cos\varphi'+\cos\psi')(\cos\varphi+\cos\varphi')+(\sin\varphi'-\sin\psi')(\sin\varphi-\sin\varphi')}{(\cos\varphi+\cos\psi)(\cos\varphi+\cos\varphi')+(\sin\varphi-\sin\psi)(\sin\varphi-\sin\varphi')}$$

$$=\frac{1+\cos(\varphi'-\varphi)+\cos(\varphi'-\psi')+\cos(\varphi+\psi')-2\sin^2\varphi'}{(\cos\varphi+\cos\psi)(\cos\varphi+\cos\varphi')+(\sin\varphi-\sin\psi)(\sin\varphi-\sin\varphi')}$$

$$=\frac{1+\cos(\varphi'-\varphi)+\cos(\varphi-\psi)+\cos(\varphi'+\psi)-2\sin^2\varphi'}{(\cos\varphi+\cos\psi)(\cos\varphi+\cos\varphi')+(\sin\varphi-\sin\psi)(\sin\varphi-\sin\varphi')}$$

$$=\frac{(\cos\varphi+\cos\psi)(\cos\varphi+\cos\varphi')+(\sin\varphi+2\sin\varphi'+\sin\psi)(\sin\varphi-\sin\varphi')}{(\cos\varphi+\cos\psi)(\cos\varphi+\cos\varphi')+(\sin\varphi-\sin\psi)(\sin\varphi-\sin\varphi')}$$

$$=1+\frac{2(\sin\varphi-\sin\varphi')(\sin\varphi'+\sin\psi)}{(\cos\varphi+\cos\psi)(\cos\varphi+\cos\varphi')+(\sin\varphi-\sin\psi)(\sin\varphi-\sin\varphi')}$$

$$=1+\frac{2(\sin\varphi-\sin\varphi')(\sin\varphi'+\sin\psi)}{(\cos\varphi+\cos\psi)(\cos\varphi'+\cos\psi)-(\sin\varphi-\sin\psi)(\sin\varphi'+\sin\psi)}$$

two alternative forms for the denominator, which are suitable for use with the tabulated cosines, and may easily be resolved into the form already given in equation (23).

The cosine tables are available for all the operations which have been described in previous papers, provided the equations are arranged so that the cosines appear only in a term which may be regarded as a correction to the paraxial result. Such equations can always be found in a form convenient for calculation. The positions of the focal lines, for example, may be determined in this way. When it is desired to find errors of phase in the emergent wave front the equation given previously

$$\Sigma\frac{(h'-h)\mu\sin\varphi}{\{(1+\cos\varphi)(1+\cos\varphi')(1+\cos\psi)(1+\cos\psi')\}^{\frac{1}{2}}},$$

is probably as suitable as any other form, and fits very conveniently into the present system of calculation.

The author desires, in conclusion, to thank Miss Gillman for checking the table of cosines, and both Miss Everett and Miss Gillman for comparative calculations of rays in various optical systems which have indicated the degree of accuracy obtainable as recorded in the paper.

ABSTRACT

Equations for tracing rays in an axial plane through an optical system have the normal refraction terms separated from those representing aberrations. By expressing the latter as a fraction with the

first order aberration as the numerator and a correcting factor, which may take various forms, as the denominator, rays may be traced exactly through the system using a short table of cosines in terms of sines in place of the extensive tables, giving sines in terms of angles generally employed. A considerable saving of time is effected in the calculations, and the estimation, without calculation, of the aberrations of other rays is facilitated.

DISCUSSION.

Mr. J. W. French communicated the following note: I have read with much interest Mr. Smith's very valuable contribution to the subject of optical computation. The principal advantage claimed for the system over, say, the Steinheil and Voit trigonometrical system, is, I understand, one of speed, as Mr. Smith's system does not involve the conversion of functions of angles to and from degrees and minutes. In order to test this point, I have had a cemented objective computed by both methods, the same operator and machine being engaged throughout. It would appear that the number of operations under Mr. Smith's system is much greater than in the case of the Steinheil system. Regarding each item that is recorded on paper by the computer when using a machine as indicating an operation, then for the complete objective the number of operations involved in Mr. Smith's method is 69, as compared with 33 in the Steinheil method. By the use of a machine having an integrating slide, it is possible that the number of paper entries involved in Mr. Smith's method could be substantially reduced, but the number of actual calculating operations would still appear to be considerably greater than in the Steinheil method. Thus, the number of additions or subtractions per surface on the Smith system is eight, as compared with four in the Steinheil system; the number of multiplications or divisions on the Smith system is eight, as compared with five on the Steinheil; the number of table references on the Smith system is three, as compared with three on the Steinheil. Now, with regard to addition, subtraction, multiplication and division, it is possible that Mr. Smith adopts short cuts which are not indicated in the Paper. If this is not the case, it would appear that the Steinheil system is nearly twice as quick as the Smith system, so far as these operations are concerned. With regard to table references, it is stated that in the trigonometrical method four references to tables are involved. I suggest that really only three table references are involved. It is true there are four angles, but in the case of an objective, at all surfaces after the first, one is known. When the object is at infinity there are only three angles involved, even at the first surface, since one of the four is zero. When the object is at a finite distance there are four angles involved at the first surface, and only three at succeeding surfaces. So far as table reference is concerned, it would appear that the only advantage that remains is that Mr. Smith, by an ingenious device, is able to obtain accurate results from four-figure tables, whereas tables of six or seven figures are required in the Steinheil method. It may be of interest to note that it is our general practice to use radian tables. In view of the small number of table references and the great difference between the two methods so far as time occupied in the other operations is concerned, and considering the facility which a trained computer acquires in the use of tables, I suggest that the Steinheil system is more rapid, unless, as already stated, Mr. Smith may employ a number of short cuts that have not been indicated. So far as accuracy is concerned, the proposed method appears to be all that Mr. Smith claims. In computing an ordinary telescope objective, the final angles obtained by the two methods corresponded to one unit in the sixth significant figure and the intercepts to three in the fifth figure.

Commander T. Y. Baker (communicated): The computing opticians' methods of ray tracing, although varying in detail as to conventions of signs, have all been practically of the same type—namely, successive calculations of the refractive equation $\mu \sin\varphi = \mu' \sin\varphi'$, and successive transference from one surface to the next usually by formulæ connecting the crossing point of the ray and the aperture of the point of its inter-section with the next surface. Such formulæ have all been purely trigonometrical, and, as Mr. Smith states, the calculation $\psi' = \psi - \varphi + \varphi'$ crops up for each surface.

Optical computation by such methods is slow. Probably, at the best, one man working alone cannot exceed a speed of 12 surfaces per nour, even with the use of a calculating machine to replace logarithmic computation. The slowness is mainly caused by the constant necessity of referring to tables of sines and the need for interpolation between tabulated values. A small improvement can be made if sines tabulated to seconds are available, although the greater bulkiness of the tables makes in itself for delay. It must, therefore, be a great pleasure to the calculating optician to find a scheme put forward, such as the present one, in which the troublesome parts of the ordinary methods are circumvented. In this scheme of work, the actual values of the angles of incidence, refraction or of inclination with the axis, never need to be found; their sines only are dealt with except in one place, and the whole set of formulæ could be expressed in an algebraic form without the use of a single trigonometrical function. Except for the tabulated values of $\sqrt{1-x^2}$, which is really the meaning of Mr. Smith's table, nothing is needed except ordinary logarithms, and not even those when the multiplications and divisions are done by calculating machine. I have had the opportunity, through Mr. Smith's kindness, of testing the speed of his scheme against that of ordinary methods, and, although nothing like full proficiency has yet been attained, we have, nevertheless, already reached the "five minutes per surface" mark, and it would appear as if the ultimate limit would be from 20 to 25 surfaces per hour. The slowest part of the scheme is the solution of equation 23, in which h' is determined from h; in fact, this equation takes nearly half the time. Possibly on a different type of calculating machine, where numbers could be left on the machine for subsequent use, one might be able to economise time; but, with the machine that I was using, I found that equation 23 as it stands is not in the most suitable form, and prefer to use the following, which is readily established by geometry, and is not approximate in any form:—

$$h' = h\,[1 - 2\sin\tfrac{1}{2}(\varphi'+\psi)\sin\tfrac{1}{2}(\varphi'-\varphi)\sec\tfrac{1}{2}(\varphi+\psi)].$$

The calculation of the expression

$$2\sin\tfrac{1}{2}(\varphi'+\psi)\sin\tfrac{1}{2}(\varphi'-\varphi)\sec\tfrac{1}{2}(\varphi+\psi),$$

which is really of aberrational magnitude, can be made with sufficient accuracy by a specially constructed slide rule based on the following principles: If on a circle of unit radius three points be marked off on the circumference from a common zero, and subtending the three angles φ, φ' and $-\psi$ at the centre, the fraction above can be written

$$\frac{\text{Chord } (\varphi' \text{ to } -\psi) \times \text{chord } (\varphi' \text{ to } \varphi)}{2 \times \text{perpendicular from centre on chord } (\varphi \text{ to } -\psi)}$$

It remained, therefore, from the known values of the sines of the three angles to determine the chords and the perpendicular. This was easily done by constructing a scale of $\sin^{-1}x$. The actual distance, consequently, in this scale between the marked values of $\sin\varphi'$ and $\sin\varphi$ is the angle $\varphi'-\varphi$, and the chord corresponding to this angle can be read on the slide. A second slide read the length of the perpendicular. The accuracy obtainable depends, of course, on the range to which the values are marked and on the scale of

the slide rule. Probably, for separated lens systems, 90 per cent. of surfaces will be such that sin φ, sin φ' or sin ψ are all below 0·3, and if the slide is made to go up to that value, the occasional cases outside the range can be dealt with by Mr. Smith's formula or directly by trigonometrical methods. The rough rule to guide one respecting the accuracy seemed to be that if the value of the above fraction had two zeros after the decimal point, it could be trusted to be right to about one in the fifth place. If there was only one zero, it was usually desirable to make the calculation independently of the slide rule. The above accuracy can, of course, be improved by increasing the scale of the slide rule, but it appears to be sufficient for separated lens systems. I have not had sufficient time to investigate to what extent the method is applicable to photographic lenses where the curves are steep and the apertures wide. In conclusion, I feel that Mr. Smith is to be congratulated on having evolved a system of ray tracing which looks to be invaluable for the optical computer, and I trust that steps will be taken to bring it prominently to the notice of instrument making firms. I have in mind several instances of delay in getting out new designs owing to congestion in the computation section, and a scheme that will enable a computer to double his output ought not to be lost sight of.

Mr. F. J. W. Whipple (communicated): It may be of interest to place on record a variant of Mr. Smith's formula (23), which appears to have advantages from the computer's point of view as well as the merit of simple geometrical proof. If P is the point where the ray cuts the refracting surface of which A is the vertex and C the centre, let AN, perpendicular to the incident ray, cut the bisector of the angle PCA at Q. $\mathfrak{h}$ is the sum of the projections of the equal lines PQ and QA on any perpendicular to the incident ray, so that

$$\mathfrak{h} = PQ[1+\cos(\psi+\varphi)]$$

Similarly $\mathfrak{h}' = PQ[\cos(\varphi-\varphi')+\cos(\psi+\varphi')]$

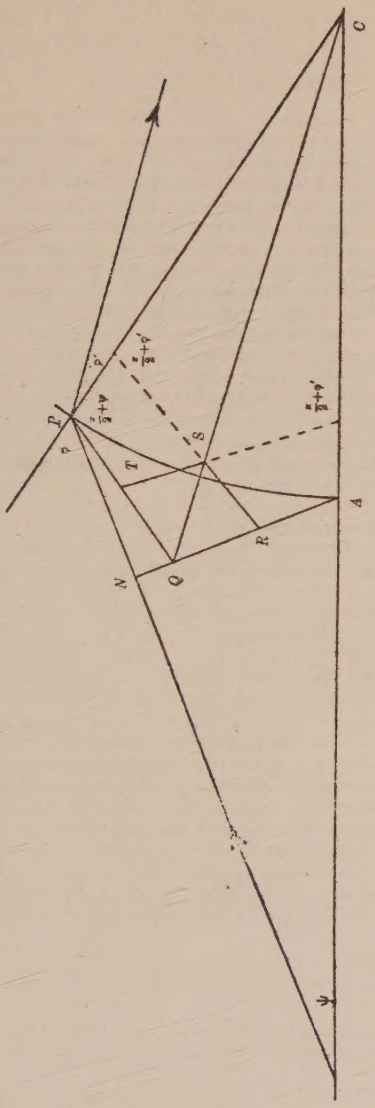
Hence
$$\frac{\mathfrak{h}'}{\mathfrak{h}} = \frac{\cos(\varphi-\varphi')+\cos(\psi+\varphi')}{1+\cos(\psi+\varphi)}$$
$$= 1-\frac{(\cos\varphi'-\cos\psi)(\cos\varphi'-\cos\varphi)+(\sin\varphi'-\sin\varphi)(\sin\varphi'+\sin\psi)}{2-\frac{1}{2}(\cos\varphi-\cos\psi)^2-\frac{1}{2}(\sin\varphi+\sin\psi)^2}$$

Evidently small errors in the cosines will exert very little influence when the ratio of $\mathfrak{h}'$ to $\mathfrak{h}$ is computed from this formula. The formula (23) itself can be derived in like fashion by considering the projections of a symmetrical linkage of four equal sides. Connecting A and P, one side of this linkage, AR, is perpendicular to the incident ray, and the next (RS), which is perpendicular to the antiparallel of the refracted ray with regard to PC, terminates on the bisector of the angle PCA; the other two are images of the first two in this bisector. On taking the ratio of the projections it is found that

$$\frac{\mathfrak{h}'}{\mathfrak{h}} = \frac{\cos(\varphi'-\varphi)+\cos 2\varphi'+\cos(\psi-\varphi)+\cos(\psi+\varphi')}{1+\cos(\varphi+\varphi')+\cos(\psi-\varphi')+\cos(\psi+\varphi)}$$

The required formula can be deduced at once. The highest precision would be obtained by using it in the form

$$\frac{\mathfrak{h}'}{\mathfrak{h}} = 1+\frac{4(\sin\varphi-\sin\varphi')(\sin\varphi'+\sin\psi)}{8-(\cos\varphi-\cos\varphi')^2-(\cos\psi-\cos\varphi')^2-(\cos\psi-\cos\varphi)^2}$$
$$-(\sin\varphi+\sin\varphi')^2-(\sin\varphi'-\sin\psi)^2-(\sin\psi+\sin\varphi)^2$$


C
A
P
P'
S
T
R
Q
N

The AUTHOR, in reply to Mr. French, stated that an appreciable gain in speed had been found in spite of the greater number of operations involved. These could be divided into two classes, the first concerning quantities which had to be known to the same degree of accuracy as was ultimately to be attained, the second concerning quantities where a much rougher approximation could be adopted with impunity. About two-thirds of the operations related to the latter group. The calculation of the denominator in equation (23) could frequently be shortened when a paraxial ray had already been traced by noting that in the absence of aberration

$$\sin \psi_{\lambda-1} - \sin \varphi_\lambda + \sin \varphi_\lambda' = \sin \psi_\lambda.$$

Unless $\sin \psi_\lambda$ were large the square of the first quantity would not be appreciably affected by neglecting the aberration. The work could be so arranged that the individual contributions to the denominator need not be recorded separately, the denominator as a whole being calculated as the sum of two products obtained automatically by not clearing the product slide between operations. In the arrangement followed by the author, 21 paper entries would be made in calculating a ray by the new system through a cemented doublet, against 26 on the Steinheil system. The number of entries in the former system can be further reduced, but, in general, this is inadvisable, as all the numbers retained involve results of value to the computer. For example, the value of the denominator gives the relative magnitudes of the aberrations of the first and of higher orders. No detailed procedure had been suggested because this would necessarily vary with the type of machine used, as well as with the amount of information ultimately required from the calculations. Although emphasis had been laid on speed, the new system offered advantages in that the operations reflected the physical events which took place at the surface, and depended on its curvature simply, while in the Steinheil and most other systems the calculation hinged around the positions of the centres of curvature; and, further, it was usually possible to judge very well without calculation how a host of intermediate rays would be affected owing to the separation of the aberration terms from those relating to the normal refraction, a point in which the ordinary methods of calculation were notably deficient. In at least one more respect the new system enjoyed an advantage over Steinheil's. The same formulæ cover every case, and are uniformly reliable. On the other hand, with Steinheil's, a number of special cases arise in which entirely distinct formulæ must be adopted if the numerical accuracy is to be maintained. With the new method there would be no great difficulty in arranging a machine to carry the calculations automatically through a number of surfaces when the optical data and the initial ray co-ordinates were given.

Mr. Whipple's geometrical proofs were very ingenious and interesting, and the rearrangement of the denominator, though ordinarily troublesome, might be advantageous when the full accuracy of the cosine table was called for. Commander Baker's equation was of a somewhat different type from those considered in the paper. Apart from slide rule methods it was evident that linkages could be contrived to give correcting factors to the first order aberration automatically, and these might well be found to give the most rapid means of tracing rays.

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